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THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.,

AND

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(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

CANCER.—*Electrolytic Treatment of.*—Virchow believes that cancer is at first purely local; and Waldeyer believes it always originates from epithelium. Cases are related in which the following treatment dispersed the tumours in the early stage, and when the parts affected were not adherent to the deeper parts. Even in such cases however the plan may be tried. The method of applying the agent is described as follows:—"Suppose, in the first place, that the tumour is of moderate size, hard, and of slow growth. With or without chloroform, as circumstances may determine, two, three, or four needles are introduced at equal distances into the tumour, and connected with the negative pole or cathode of the battery. The positive pole or anode is connected with a broad plate, which is applied with a wet compress to the skin at a certain distance from the tumour, and moved from place to place. The current is gradually increased up to thirty-five or forty elements, and after twenty to thirty minutes, gradually lessened, and needle after needle withdrawn. This operation is repeated for a limited number of times at intervals of three or four days, the needles being introduced at different parts of the tumour. After this energetic treatment, a weak current of four to eight elements is to be used daily for months. The negative pole, formed of a flat plate and a wet compress, is placed over the needle punctures, and the positive at a distance from the tumour as before. The sloughs produced by the needles come away after a time, and their track serves as a conductor for the current. If, in the second place, the tumour be a large one, then the only modification is that, during the operation, needles should be extracted and placed in different situations. The number of the needles should not, however, be increased above four at one time. If, in the third place, the tumour be large and ulcerating, then an attempt is made to produce sloughing of it. In this case, the anode (or positive) needle is thrust into the centre of the

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tumour, and its base transfixed with a number of needles connected with the negative pole. These are left for about twenty minutes, and then inserted in a different position. In from seven to ten days the whole or part of the tumour generally sloughs away, and if it is not entirely removed, the operation may be repeated. After this rather severe treatment, there is never any fever or special constitutional disturbance. In this case, also, after the tumour has sloughed, the treatment with weak currents should be continued for months. Of late, the author has sometimes introduced a slight difference in the treatment of the first and second class of cases. Instead of inserting cathode needles throughout, he has occasionally begun with an anode needle, which he has only kept in for a short time, and he thinks this an improvement on the original plan. The anode needle should only be kept in for a short time, as otherwise it is apt to cause sloughing; after this single application of the positive needle, he continues as before. It is to be noted that the author attaches great importance to the efficient carrying out of the treatment with weak currents, after the energetic currents have been used preliminarily; and he generally entrusts this latter treatment to the patient. The author has also used a similar mode of treatment, as others have done, in aneurisms, varix, &c. His method is in this respect different from the usual, that he does not insulate the stems of the needles which he introduces." (M. Neftel, p. 60.)

CHOLERA.—*Chloralum.*—Dr. Black recommends the internal administration of chloralum (chloride of aluminium) in cholera. It is a powerful disinfectant, and, unless the success which certainly appeared to follow its use in really desperate cases in India was merely coincidence, it is a remedy of great value, and acts by destroying the poison in the system. It is free from smell and taste. A mixture of one part of liquid chloralum must be made with twenty-five of water, and an ounce of this should be given every half-hour, and an enema containing sixteen ounces of the mixture should be administered every hour. Undiluted chloralum should be sprinkled upon and around the bed. (Dr. H. Blanc, p. 63.)

FEVERS.—In all fevers attended by the action of weak hearts, try digitalis as a heart tonic. Dr. T. W. Grimshaw has especially attracted our attention to this remedy. He sometimes gives as much as one ounce and more of the *infusion* of digitalis every three hours for five or six days together. The heart seems to have been even invigorated by it. Dr. Anstie thinks that it sustains the exhausted heart, checks the

too rapid pulse, and in short enables the heart to hold on till the course of the fever changes. (Dr. F. E. Anstie, p. 1.)

GOUT AND GRAVEL.—The uric acid diathesis is a disease of sub-oxidation. Uric acid can be converted into urea by the action of oxygen, and this process is always going on in the body. When, however, the oxidation is imperfect, part of the uric acid, a larger part than natural. is unoxidised, and gout or gravel result. The etherial solution of peroxide of hydrogen, commonly known as ozonic ether, affords us a mode of supplying oxygen, and it is found that this drug gives us a mode of successfully treating gout and allied diseases. The dose recommended is half a drachm to a drachm of the ether in a wineglassful of water three times a day or oftener. (Dr. Day, Med. Times and Gazette, May 24, p. 554)

INTERMITTENT FEVER.—*Treatment of by other Cinchona Alkaloids than Quina.*—It is found, as the result of extensive experiment, that the cinchona alkaloids—cinchonia, quinidia, and cinchonidia possess considerable therapeutic value; and, as they are obtained very largely in the manufacture of quinia from some common barks, their practical utility is of great consequence. Quinidia is the most energetic. It is found that it is most serviceable when administered during the paroxysm only. The least active is cinchonia, but even this is energetic, and, in an adequate dose, a sure remedy. The average amount of quinia necessary to cure a case of intermittent fever varies between 31 and 70 grains, of cinchonia from 54 to 76, and of cinchonidia from 43 to 90. (Dr. Dougall, Edin. Med. Jour. Sept., p. 193.)

MILK AND DISEASE.—The outbreaks of fever, the sources of which have been traced to milk infected with typhoid poison, have drawn a good deal of attention to the peculiar properties milk has of absorbing surrounding atmospheric poisons. A little milk exposed in a saucer all night near a sink will be found in the morning to possess a mawkish and unnatural odour. It is peculiarly liable to putrefy when there is a thundery atmosphere, and it is singular that veal—a meat fed on milk—will sometimes become quite decomposed under the same atmospheric or electric conditions in the course of a few hours. (Mr. H. Willington, p. 58.)

Sewage Grass.—The following conclusions seem pretty well established:—1. Sewage grass is quite as healthy and good for cattle as ordinary grass. 2. If the sewage is run on in too large quantities, it is possible that evil may follow, as the liquid matter may adhere to the grass for several inches from the ground. Even in this case no communicable dis-

ease—such as typhoid—would be given to the drinker of the milk. 3. Supposing the sewage matter to be contaminated with typhoid poison, there is no evidence that typhoid or any other form of disease can be thereby produced in animals. 4. Numbers of scientific witnesses are unanimous as to the excellent character both of milk and butter produced from sewage grass. (Editor of Medical Times and Gazette, p. 9.)

TYPHOID FEVER.—Two important facts appear to have been established comparatively recently:—1. That typhoid fever may be generated *de novo* from habitual drinking of water contaminated with sewage, or from exposure to gaseous emanations from sewers. Dr. Murchison cites numerous instances of sporadic and limited epidemic outbreaks of enteric fever, the spontaneous origin of which in sewer emanation, and in contaminated drinking water, was proved to a certainty, there being no proof of previous pollution with enteric excreta. 2. That milk possesses such absorbent properties that if merely exposed in an open vessel in the neighbourhood of a typhoid case, it may become a vehicle for the poison. (Dr. J. Dougall, p. 45.)

Try the sulpho-carbolate of sodium, on the principle of Dr. Sansom's antiseptic treatment of small-pox. Give 20 grains every four hours to an adult, and gradually increase during the next few days to 30 grains. Beyond this you may produce cerebral symptoms. Children require much smaller doses, and may have very small doses of Batley's liq. opii sed. to assist in checking the diarrhœa. If possible, give the remedy during the period of incubation. (Dr. S. Skinner, p. 30.)

AFFECTIONS OF THE NERVOUS SYSTEM.

FACIAL NEURALGIA.—*Aconite.*—Aconite is a powerful local remedy, acting not only as a controller of morbid sensation, but as a retarder of vascular action, and by consequence as a modifier of muscular action. Small quantities of the drug are readily absorbed through the skin, attack the nerves and muscles in their neigbourhood, but show little disposition to be absorbed into the general current of the circulation. In cases of facial neuralgia it is of the greatest use. (Dr. F. Bagshawe, p. 70.)

HYSTERICAL HYPERÆSTHESIA.—Nothing is more useful in cases of hysterical hyperæsthesia than faradising the part pretty strongly. It should be applied unsparingly. Supposing, for instance, that the hyperæsthetic area is on the skin, dry

conductors should be used, one being placed five or six inches distant on an indifferent spot; the other (which should be in the form of the wire brush) should be so used as to apply a kind of electrical flagellation to the whole of the affected area. This must be kept up several minutes with a powerful current; and if the painfulness of the proceeding becomes intolerable, it is better to give the patient a little chloroform and go on, than to do the thing by halves. Faradisation is of no use in true neuralgia. (Dr. Anstie, p. 93.)

LEAD POISONING.—People may be poisoned by water passing through a lead pipe from a well to the house. We suspect this may be the case in towns, and when the water is particularly soft and pure the danger may even be increased. It is suggested by Dr. Medlock in such cases to have block tin pipes. (Dr. J. W. Von Tunzelmann, Dr. Medlock, p. 16.)

Collateral evidence and argument in every way demonstrate the superiority of hard against soft water. Soft water is very capable of absorbing lead, and at one time in Glasgow there was a good deal of lead poisoning and colic from this source. (Editor of Med. Press and Circular, p. 13.)

MIGRAINE.—Migraine may be artificially induced, as the result of injury, and when so originated may recur for years, just as does ordinary migraine. In a case of this nature in the person of a medical man great benefit was always experienced from taking a cup of strong tea at the commencement of the attack, also from a tumbler of sherry wine; but the remedy *par excellence*, the patient found in guarana. Four doses of thirty grains each taken in two hours afforded such relief that the attack which foreboded to be of unusual severity turned out to be a very small affair indeed. (Dr. D. Morton, p. 78.)

Give three grains of guarana whenever the attack comes on, and in one or two hours, if not relieved, take another dose. (Dr. Macdowell, p. 80.)

NEURALGIA AND PARALYSIS.—*Electricity.*—Electricity may be of great use. Faradisation may do good occasionally, but the constant voltaic current is the great thing in cases of pain. Both the electrodes ought to be firmly pressed upon the skin and held there, neither of them being moved about; "that is, the current must be voltaic, and must be *constant*, not interrupted; and the electrodes must be applied so as to include in the circuit the affected nerves." The number of cells used must be according to the feelings of the patient; they should be as many as can be borne without pain. In the face five cells will be enough to begin with, but fifteen or twenty cells may be required; elsewhere more cells may

be used. One of the electrodes should be placed over the point of origin of the nerve, and the other over the painful spot. The direction of the current seems not to be so important, but it should be used gently and steadily.

Paralysis.—If *cerebral*, be careful not to employ it too soon; wait some months before trying it, till all head symptoms subside. First, test the excitability of the muscles by the faradic and interrupted voltaic currents. The amount of contractility the muscles show will tell what good may be obtained. "If the contractility is normal, though voluntary power is abolished, electricity will do nothing towards restoring the power of the will over them; if the contractility is somewhat, but only slightly diminished, the condition of the muscles may be improved; and when the contractility is found to be markedly reduced, but not abolished, great good may be achieved." Having tested the muscles, "treat them by means of the current to which they most readily answer; or employ the interrupted voltaic current where the excitability is normal, and the faradic where it is diminished."

"If *voltaism* is employed it must be by the interrupted, not the continuous current. Large-sized sponges should be used, well wetted with salt and water, and one (the upper one of the two, that is, the nearest to the central organs of the nervous system) being kept steadily on one spot, the other should be moved slowly down along the course of the muscles; so that every part of each of them is successively brought into the circuit of the current, and by the movement of the lower sponge the current is *interrupted*. It does not matter whether the current is direct or inverse; only it must be remembered that the direct current acts more strongly on the muscles than does the inverse, and that the current must not be so strong as to excite pain." This rule also applies to faradisation.

"If the *faradic* current is used, sponges or metallic buttons covered with chamois leather are to be used; and they must be *well* wetted, or the current may only irritate the skin, instead of penetrating to the muscles."

In *traumatic paralysis* faradisation is invaluable. The muscles will often remain paralysed after the injured nerve or spinal cord has recovered, but faradisation will act wonderfully. The faradisation, will, of course, be of no use till after the injured nerve has regained its power of conduction; but in recent cases of injury the interrupted voltaic current should be localised in the affected muscles, and direct faradisation be employed when farado-contractility begins to return.

"In cases of *local paralysis*, dependent on cold or on metallic poisoning, as from lead or mercury, or on diphtheria, the employment of electrisation should not be neglected when the affection has resisted other means of treatment. In these cases the faradic and the interrupted voltaic currents, used alternately and localised in the affected muscles, will generally effect a cure. As a rule the farado-contractility is unaltered in muscles paralysed by exposure to cold; but this is not the case in facial paralysis from cold. Here the farado-contractility is diminished or quite lost, while the excitability to the interrupted voltaic current is increased; and faradisation may be employed for weeks without effecting any improvement. In this affection, therefore, the interrupted voltaic current must be used and localised in the muscles till the farado-contractility is restored, and then the two forms may be employed alternately; and in this way cases of even years' standing may be cured or greatly benefited." (Ed. of Med. Times and Gazette, p. 84.)

Neuralgia.—You must understand the difference between the galvanic and faradic currents. For neuralgia the *galvanic* current is the best, although faradisation sometimes answers; a rough, harsh current does no good, but a rapidly interrupted and pleasant current will do much better—"better to use a mild current a long time than a strong current a short time." The direction of the current has been too much trusted in. Pain may be relieved and other benefits obtained by the passage of the current through the body by either pole, and in any direction. In this way you may treat migraine, facial neuralgia, gastralgia, sciatica, and other painful nerve affections. (Dr. G. M. Beard, p. 99.)

On using the constant current for the relief of trigeminal neuralgia it is unimportant whether the current is passed from centre to periphery, or the other way. The relief obtained is just the same whether the positive pole is placed on the nape of the neck, or on the focus of pain on the face. (Mr. S. Craddock, p. 107.)

NEURALGIA.—*Phosphorus.*—Further experience of the use of phosphorus in cases of neuralgia confirms the opinion that it is a remedy of great value. We should commence by giving $\frac{1}{16}$th or $\frac{1}{12}$th of a grain every four hours. Unless this gives decided promise very little good will be gained by increasing it. Varous solvents may be employed, but, perhaps none is better than absolute alcohol, which dissolves ten grains to every 1000 drops by the aid of heat. Chronic cases are cured the least rapidly, when they are cured at all; while among them is found the only absolute failure of the

remedy employed. Muriate of ammonia administered in doses of from one to two drachms every two or three hours, ranks next to phosphorus in efficacy; but it must be given in doses as large and frequent as these. (Mr. J. A. Thompson, p. 110.)

TOOTHACHE.—*Electricity.*—Pass a continuous current from the neck through the side of the jaw which is painful. The negative pole is to be placed on the anterior lateral portion of the neck, and the positive pressed on the cheek over the painful tooth. (M. Bouchaud, p. 87.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

WOUNDS AND ANEURISMS OF THE VERTEBRAL ARTERY.—Wounds and aneurisms of the vertebral artery are particularly liable to be mistaken for similar affections of the carotid, and no less than seven cases are recorded in which the carotid was tied for vertebral aneurism, and four for wounds of the vertebral artery. The reason of this mistake is, that pressure on the carotid for two inches above the level of the clavicle arrests the circulation through the vertebral artery also, which lies immediately behind it. As, however, the vertebral artery passes into the foramen of the transverse process of the sixth cervical vertebra, it is not compressed by pressure on the carotid above the level beforenamed, and in this fact rests our power of diagnosis between wounds and aneurisms of the two arteries. Ligature of the carotid for wound of the vertebral artery is attended by the most disastrous consequences, as the brain is deprived of its supply of blood, and the remedy used tends to aggravate the disease it was meant to cure. (Mr. T. Holmes, p. 203.)

EPISTAXIS.—In a case of epistaxis related by Dr. Leeper in which the ordinary mode of plugging seemed inapplicable as the patient was almost dying, the hemorrhage was arrested by passing into each nostril two medicated suppositories of perchloride of iron, each containing two grains. A bit of dry lint was then placed on the anterior nares. Ergot was administered internally. (Dr. W. Leeper, Dublin Journal of Medical Science, Nov., p. 364.)

WRITER'S CRAMP.—Patients in whom symptoms of writer's cramp have appeared, should always be warned that they will never be able to perform an excessive amount of writing, and that any attempt to do so may be followed by disastrous consequences. Unfortunately the disease rarely comes under the eye of a physician till it has existed months

or years, and has made considerable advances. The application of the continuous current of electricity to the weakened muscles is of the greatest service, and is preferable to Faradisation. The positive pole should be placed over the brachial plexus in the axilla, and the negative in a basin of warm water in which the hand is immersed. Voluntary movement of the hand is to be practised simultaneously. (Dr. W. A. Holles, Practitioner, Aug., p. 85.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

HÆMOPTYSIS.—*Hypodermic Injection of Ergotine.*—We possess no remedy equal to ergotine as a remedy for hæmoptysis. The prompt suppression of all oozing into the air-cells which, in view of the danger of phthisis in *all hæmoptoe*, it is so important to effect, can probably be in no way so effectually accomplished as by the employment of this agent. It acts the most rapidly and effectually when administered subcutaneously, for which purpose Bonjean's ergotin is the best. (Dr. Anstie, p. 130.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

DIARRHŒA OF CHILDREN.—Oxide of zinc is a remedy of value in children's diarrhœa. It should be given in doses of two grains every three hours, mixed with mucilage and a little simple syrup. (Mr. W. Berry, Practitioner, Nov., p. 356.)

FLATULENT DISTENSION OF THE ABDOMEN.—The most important point in the treatment of this affection is to select such kinds of food as give the greatest amount of nourishment, yet tend the least to produce flatulence. Let the patient take no bread, but eat biscuits instead; take no solid vegetables, but use lemon juice freely; shun soups, cucumbers, and fruit. Prefer meat, fish, fowl, or game—tender, but perfectly fresh; take no rich sauces, or meat twice cooked, or salted or seasoned. (Dr. F. Sibson, p. 133.)

Puncture of the Bowel in Extreme Flatulent Distension.—In cases of extreme flatulent distension, we need not fear puncturing the bowel with a fine trochar in order to give relief. The results of this practice are most satisfactory, as shown by a case in which it was repeated four times. The canula used should be three or four inches long, otherwise as the belly falls it will slip out of the gut, and moreover a considerable thickness of abdominal walls has sometimes to be traversed before the bowel is reached. Not the least pressure should be

made upon the abdominal walls, as one may be tempted to do when the gas is nearly exhausted, because there is a liability to squeeze out a little of the fluid contents of the intestine. Care should be taken to puncture that part of the intestine which is highest as the patient lies in bed, otherwise a jet of liquid fæces may be shot out with the first sudden explosion of gas. The chances of extravasation of any fluid into the peritoneum from the fine punctured wound are very small. The puncture merely separates the muscular fibres, and on the withdrawal of the canula the muscular planes immediately readjust themselves, or a protrusion of mucous membrane takes place, which entirely prevents the escape of the intestinal contents. (Dr. H. C. Cameron, p. 122.)

LIVER AFFECTIONS.—*Carlsbad Water.*—The hepatic disorder most likely to be benefitted by the Carlsbad cure is hyperæmia. This usually results from overeating and drinking, and especially much indulgence in rich articles of food, without sufficient exercise. There is generally dyspepsia, sallowness of skin, bilious vomiting, and a sense of uneasiness and fulness in the region of the liver. Fatty liver may be actually cured by this treatment; but if the pulse is irregular or there is tendency to syncope or vertigo, "the cure" will be contra-indicated. For gall stones and biliary concretions the Carlsbad waters are considered almost a specific. (Dr. S. H. Ward, Lancet, Aug. 2, p. 145.)

HERNIA.—*Radical Cure.*—After an operation for strangulated hernia, there is no reason why something should not be attempted to prevent a return of the protrusion. The real danger arises rather from the state of the strangulated bowel than from any way of dealing with the peritoneal sac and parietes after the strangulation has been relieved. The best mode of carrying this out is, after relieving the strangulation, to bring together the sides of the sac and the aponeurotic structures by means of a handled-curved hernia needle armed with silver wire. That this operation is successful in permanently curing cases of hernia is proved by the instances related. (Mr. J. Wood, p. 214.)

A new method of Operating in certain cases of Strangulated Hernia.—Mr. Annandale relates a case of large strangulated umbilical hernia in which he divided the abdominal parietes to the extent of three inches in the mesial line between the umbilicus and ensiform cartilage. The gut was easily removed from the sac by traction from within, and the firm ring at the neck of the sac was then divided freely by a probe pointed knife introduced through the wound. The bowels

were freely moved three hours after the operation, but the patient remained exhausted and died from a fatty condition of the heart forty-eight hours after the operation. This operation is by no means intended to supersede the ordinary plan, except in certain cases, viz., umbilical hernia of large size, especially if the coverings are thin or ulcerated; 2nd, large scrotal herniæ which have been recently reducible; 3rd, other herniæ in which the usual operations cannot satisfactorily be performed. (Mr. T. Annandale, p. 220.)

ULCERATION OF THE RECTUM.—There are two forms of ulceration of the rectum. Syphilitic ulceration is the more common. It is most frequently met with on the dorsal aspect of the rectum, and occasionally extends completely round the bowel, within a couple of inches of the anus. This form of ulceration is said sometimes to occur as a result of dysentery; it is found occasionally without any obvious cause. There is a constant wearying pain and profuse sanious discharge. It must be treated on the same principles that we should treat syphilitic ulceration of the fauces, viz., mercurial applications locally and iodide of potassium internally; with the object of checking the discharge sulphate of copper lotion may be used as an injection. For ordinary non-syphilitic ulceration it is good practice to apply some caustic directly to the part. Nitrate of mercury lightly wiped over the ulcer with a brush is the most suitable. (Mr C. Heath, p. 230.)

AFFECTIONS OF THE URINARY ORGANS.

ACUTE BRIGHT'S DISEASE.—In a large proportion of cases, rest in bed, with a scanty diet and a liberal use of diluent drinks, will suffice for the cure. A purely milk diet is very suitable. It may be taken cold or tepid, from half a pint to a pint at a time. An adult will take sometimes as much as a gallon in the twenty-four hours. Purgatives should not be ordinarily employed; they should be reserved for cases in which there is an excessive and increasing dropsical effusion which does not yield to other means of cure, but more specially for cases in which cerebral symptoms, the result of uræmia, are either present or apparently impending. Warm baths are of the greatest service, but the wet sheet and blanket bath is still more efficacious. A sheet is wrung out of warm water, and the patient is enveloped in the wet sheet up to the neck, then three or four dry blankets are closely folded over the wet sheet. The advantage of this kind of bath is that its action may be so much prolonged. When the wet pack is removed, the patient should be quickly

rubbed dry, and enveloped in blankets. If the renal congestion is excessive, as shown by the scanty secretion of highly albuminous urine, with vomiting, headache, and other threatening nervous symptoms, local bleeding by leeches or cupping on the loins is often extremely useful, and is quickly followed by an increased secretion of urine. (Dr. G. Johnson, p. 151.)

ACUTE CYSTITIS SIMULATING ACUTE BRIGHT'S DISEASE.—In a large proportion of cases acute cystitis, when not excited by any obvious cause, such as stone, stricture, or gonorrhœa, follows directly on a chill. The symptoms are frequent micturition, with more or less of uneasiness or pain in the region of the bladder. The urine is acid, and contains albumen, partly derived from blood, and partly from pus contained in the secretion. It is, however, of normal specific gravity, and is albuminous only in direct proportion to the amount of blood and pus which it contains, and the most careful microscopic observation discovers no tube casts. Confinement to bed or a warm room is necessary. Liquid food, without stimulants, should be given. A warm hip bath night and morning, and a dose of morphia at bedtime to allay irritation and procure sleep, are also of service. If a chronic catarrhal condition of bladder remain, there is no remedy like copaiba, of which one to three capsules should be given three times a day. (Dr. G. Johnson, p. 148.)

CALCULI.—*New Instrument for Removing Débris from the Bladder.*—Mr. William D. Napier has invented a most ingenious instrument for this purpose. It consists of a metallic tube like a catheter, containing an india-rubber stem completely filling it, but capable of being pushed out beyond it. It is introduced into the bladder, and the stem pushed forward a little, when the terminal portion expands into a cup-shaped form, very like a convolvulus flower. This, when it is retracted, embraces any débris of calculous matter which may have gravitated into it, and so protects the delicate mucous membrane of the urethra as it is withdrawn from the bladder. When the india-rubber cup is retracted it forms an olive-shaped terminal projection from the metallic tube. (Mr. W. D. Napier, p. 265.)

Napier's Calculus Detector.—This is an instrument like a sound in appearance, but having the terminal portion black. It is not stated what this terminal portion is made of but the black pigment is produced by momentary immersion in a solution of nitrate of silver. It is intended to afford evidence of the presence of a calculus by ocular instead of aural proofs—the outer substance being so peculiarly sensitive and readily

impressed by contact with any hard particle, however minute, that evidence of the existence of calcareous matter is obtained on its withdrawal from the bladder by the impressions resembling slight scratches that are found on its surface. (Mr. W. D. Napier, p. 264.)

INDIA-RUBBER CATHETERS. — The value of india-rubber catheters is not generally understood. In cases of prostatic disease they are invaluable. They are sufficiently strong to be passed along the urethra without bending, and yet so flexible that they adapt themselves to every curve of the urethra. They can be passed as easily by the patient as by the surgeon. When once introduced they may be retained in position by inserting a hollow plug into the end, which plug has two small rings one at either side through which tape can be passed. The hollow plug is closed by a stilet which stiffens the first three inches of the catheter, and is removable when the patient wishes to pass water. The retention however of the softest india-rubber is not desirable if it can be avoided. It is better to pass the catheter as required. The necessity of retention must be measured by the difficulty experienced in introduction. (Mr J. Hutchinson, p. 266.)

LITHOTOMY.--Mr. Gutteridge performs this operation in the most rapid manner as follows:—"Kneeling before the patient when in the lithotomy position, and with his eye thus in a line with the perineum, the operator first passed the staff, which he handed to his assistant; he then made very firm pressure with the fingers of his left hand upon the anus, thus dragging the skin of the left side of the perineum, with the lower extremity of the gut, well over towards the right tuber ischii. He next rapidly plunged the scalpel into the groove of the staff at a point a little to the left of the raphé, and on a level with the anterior extremity of the tuber ischii, and then, without running the scalpel along the groove, he made it cut its way through the soft tissues. Next he passed the beak of the cystotome into the roughened groove, and with the cutting edge directed a little upwards from the horizontal direction, he passed it onwards through the prostate and then turned the edge downwards and brought it out at the lower extremity of the skin-wound. This done, he passed his left index finger into the bladder, felt the stone, then withdrew the staff, and passed in along the finger a grooved conductor, much like a narrow blunt gorget, and along this, after withdrawing the finger, he guided the forceps, only one blade of which was *in* the groove of the conductor. The stone was seized and at once withdrawn, and the operation thus completed." (Mr. Gutteridge, Med. Times and Gazette, Nov. 15, p. 548.)

Mr. Davidson, of Antananarivo, Madagascar, describes the mode of operating for stone practised by himself and colleagues at the hospital of that place. A bilateral external incision is made, deeper on the left than the right side. The staff used resembles Buchanan's, but is curved a little at the angle. When the bladder is reached a guide is run along the groove of the staff into the bladder and the staff then withdrawn. The two halves of the guide are then separated as much as the size of the stone demands by a screw in the handle, and then serve as a conductor for the forceps. The stone is then extracted in the usual way. Should the calculus be very large the knife may be applied to the right side of the prostate, thus making a bilateral incision. This is seldom required. Although a bilateral external incision is made, a unilateral one only is made in the prostate at first. (Dr. A. Davidson, p. 234.)

RETENTION OF URINE AND STRICTURE.—*New Instrument for.*—Mr. Teevan describes an instrument used by him in cases of strioture and retention of urine. It consists of an exceedingly fine whalebone guide or bougie, over which is passed a No. 1 silver catheter, open at the end, and grooved on its convex side from the proximal end of the curve for about three inches towards the handle. This groove is made by cutting out a piece of the catheter. The catheter is threaded on to the guide, but the guide only runs in the interior of the catheter from the point as far as the slit. The instrument much resembles one in use in America, invented by Professor Gouley, but differs from it in some important particulars. (Mr. W. F. Teevan, p. 251.)

STRICTURE OF THE URETHRA.—One mode of treating this affection, on the principle of giving the urethra rest, is as follows:—Let the patient retain his urine, and place him in the lithotomy position under chloroform. Place the left forefinger in the rectum for a guide, and open the urethra in front of the prostate to the extent of half an inch. An elastic bougie *à boule* is now to be passed from the wound towards the meatus, to see if it will pass; if not, withdraw it, and introduce into the bladder a large elastic catheter from the wound, and tie it *in situ*. In a few days try an elastic bougie down the penis, and if it pass, try a larger and a larger one; the part will soon dilate. The wound in the rectum will heal easily if the bladder be kept pretty empty, and only emptied through a catheter. (Mr. L. S Little, Mr. F. Jordan, p. 258.)

Impassable Stricture.—When no ordinary instrument can be got in, one of the smallest filiform bougies must be taken,

but if there is much induration a small whalebone one should be selected. Having placed the patient with his back against the wall, the surgeon must take his seat in front of him, and try to pass the bougie through the stricture. If a quarter of an hour's trial fail, the bougie should be withdrawn, and given that peculiar twist at the end which resembles a skeleton key. If this fails, one of Leroy d'Etiolles "bougies tortillées" can be tried. If these means fail, do nothing more for two days, and then try again. If still you do not succeed, pass a full-sized bougie down to the stricture, and keep it there for a quarter of an hour, pressing it all the time against the stricture. This distends the parts, and the mouth of the stricture often becomes open, so that a small filiform bougie can be got in. This plan very rarely fails. In stricture the urethra is not only contracted but tortuous, so that as a rule metallic instruments are not suitable for their treatment. Chloroform is of very little use in the treatment of stricture, but leeches on the perineum and a hot bath are often of great service. (Mr. W. F. Teevan, Lancet, Aug. 2, p. 148.)

Subcutaneous Urethrotomy.—If the stricture be large enough to admit a dilator you may cure by dilatation. In some cases you may fully dilate, but the stricture will soon return, and will require an operation. Some strictures again are so hard and horny, and complicated with abscesses and fistulæ, that an operation is called for as much to give vent to suppuration as to dilate the canal. In the first case you may treat by subcutaneous urethrotomy, but in the latter cases external incisions are required. Pass a small straight staff through the stricture, and divide it subcutaneously with a small tenotome, which is introduced beneath the skin through a small puncture. Next introduce a good large catheter for half an hour, and draw off the urine occasionally. On the third day the patient will be able to introduce the catheter once a day. (Mr. W. F. Teevan, p. 249.)

Holt's Dilators in Stricture of Urethra.—The use of Holt's instruments is not so free from danger as has been generally supposed. Dr. Tibbits, of Bristol, relates two cases of death following their use. Rigors and symptoms of pyæmia preceded the fatal issue. The points of interest in the post-mortem examinations were: 1. There was no purulent matter at or about the seat of stricture, but merely slight extravasation of blood into the tissues; 2. The mucous membrane at the seat of stricture was intact in both cases when examined with a lens. (Dr. R. W. Tibbits, p. 253.)

URETHROTOMY *versus* LITHOTRITY.—No doubt lithotrity in aged and debilitated subjects is often severe and dangerous, as well as lithotomy. "What is to be done when a patient is worn down by long-continued sufferings? The answer is a simple one: Select for the removal of the foreign occupant from the bladder the operation that will produce the slightest shock (and that *only a single one*) to the nervous system, at once relieves the bladder and constitution of all irritation, involves no cutting either of the prostate gland or bladder itself, and in its performances is attended *in most cases* with the smallest loss of blood, is completed in one or two minutes, entails no risk of subsequent hemorrhage, and makes but little demand upon the powers of life for its reparation. That operation, *applicable alike to stones of all sizes*, is the one to which I have previously drawn attention—'urethrotomy by the median section with dilatation of the prostate gland.' Mr. Erichsen in 1859 drew attention in the columns of the Lancet to the benefit of the 'median operation' with prostatic dilatation, but limited it to stones of small size or such as would now be dealt with by lithotrity. Long before his paper appeared, and adopting the mode of section proposed by Allarton, I had made it applicable to such cases as those referred to by Sir Henry Thompson, believing the safety of the patient, whether young or old, and particularly the latter, was greatly enhanced by the incision passing through structures that were in the main 'aponeurotic,' and from the restricted use of the knife in the deeper parts *the grave perils of the ordinary lateral operation were wholly avoided.*" (Mr. W. L. Crowther, Tasmania, p. 244.)

"Prof. Ellis has, years ago, shown that the prostate could not be dilated, and I would recommend any surgeon who has a doubt on the point to attend his incontrovertible demonstration on the subject. To say more would almost seem superfluous, but it may not perhaps be remembered that the prostate gland is encased in two jackets of white fibrous tissue, so dense and strong that the capsule is described, in Quain and Sharpey's Anatomy, fifth edition, page 1216, as 'rather difficult either to tear or cut.'" (Mr. W. L. Teevan, p. 248.)

VENEREAL AFFECTIONS.

GONORRHŒA.—*Local Remedies.*—Gonorrhœa may be treated successfully by local remedies only, and more successfully than by medicines taken internally. The injection used must be weak enough, and often enough used. A grain of

sulphate of zinc to the ounce of water, used six or eight times a day may be commenced with, and increased to two grains if necessary. If this is not answering as well as can be wished, sulphate of copper may be tried instead, in a solution of the same degree of strength as that of the sulphate of zinc. (Dr. P. Foster, Med. Times and Gazette, Oct. 25, p. 461.)

SYPHILITIC AFFECTIONS OF THE NERVOUS SYSTEM.—Syphilitic affections of the nervous system are of course comprised amongst the tertiary forms of the disease, and, according to Ricord, are to be treated with iodide of potassium. The iodide, however, is rarely given in sufficiently large doses, rarely beyond twenty grains three times a day, and very seldom so much. The patient improves up to a certain point, but does not get beyond it. Iodide of potassium should be given as freely as the bromide. It has been pushed up to ninety grains three times a day with much benefit. If there is any convulsive affection, the bromide should be combined with the iodide. (Dr. T. Buzzard, p. 68.)

FRACTURES, DISLOCATIONS, OPERATIONS, AND DISEASES OF BONES AND JOINTS.

AMPUTATIONS.—*Modification of Esmarch's Elastic Bandage.*—Before operating on limbs Professor Esmarch applies an elastic bandage from below upwards to the part to be amputated, so as to force most of the blood upwards. Mr. Cripps, house-surgeon, to St. Bartholmew's Hospital, recommends for this purpose that "a short india-rubber tube be used, not only to prevent the blood from returning to the limb, but also for the purpose of removing it in the first place. The two ends of an india-rubber tube, twenty-one inches in length, and about three-eighths of an inch thick, are bound together with a piece of twine, the whole forming an elastic ring seven inches in diameter. A grooved reel revolving between a double handle completes the necessary apparatus. To apply this to the arm, three or four complete turns of the elastic ring are wound tightly round the hand in such a manner as to include the fingers and thumb, care being taken that the turns lie even and do not cross one another. The reel is then put under the free portion of the ring connecting the upper and lower coil. The reel is passed round and round the limb in an upward direction: thus each coil is unwound from below as another is added above. In this way four tight coils of india-rubber are carried up the limb to any distance required. The degree of tightness can be regulated

with the greatest nicety by the distance the reel is drawn from the limb by the bandager. This method of driving blood from the limb answers perfectly in the arm and in the lower part of the leg; but in carrying the bandage over the popliteal space the flexor tendons prevent the artery being effectually compressed. A firm pad in the space would probably answer the purpose. To remove the bandage, it may either be unrolled by reversing the action of the reel, or the twine connecting its two ends may be cut with scissors. A very neat little reel in a holder may be obtained at Ferguson's, in Giltspur Street." (Prof. Esmarch, Mr. W. H. Cripps, p. 190.)

ANCHYLOSIS OF THE ELBOW-JOINT.—*Excision of the Extremity of the Humerus.*—In cases of anchylosis of the elbow-joint resulting from injury, the result of resection of the joint is almost always disappointing. Either too great a degree of mobility results as the union between the divided ends becomes more complete than could be desired. Dr. Patrick H. Watson, of Edinburgh, recommends that the end of the humerus only should be excised, the other bones being left intact. He has operated on six cases with very good results. A single linear incision is made on the inner side of the arm, and through this the entire expanded extremity of the humerus may be removed without interfering with any muscular structure of consequence, especially the insertion of the triceps, which is very important. More full details of the operation will be found described in the paper. (Dr. P. H. Watson, p. 177.)

ANTISEPTIC SYSTEM AT EDINBURGH.—Professor Lister principally employs three antiseptic agents: carbolic acid, boracic acid, and chloride of zinc. The advantage of boracic acid lies in this, that being much more soluble in hot water than cold the effect of soaking lint or wool in a saturated boiling solution is a copious deposit of the crystals throughout its substance on cooling and drying, and as these will be but slowly acted upon by the discharges from the wound, the lint retains for a considerable time its antiseptic properties. It is generally used as a dry dressing, overlapping a piece of the protective moistened with an antiseptic lotion. Carbolic acid is in every case where a cavity exists, employed by preference, in consequence of its volatility. Chloride of zinc is chiefly used in a solution of forty grains to the ounce of water for application to the surfaces of wounds after operating on a part where decomposition is already present. (Mr. R. J. Godlee, p. 191.)

CARBOLISED RESIN-CLOTH IN THE TREATMENT OF WOUNDS.—An antiseptic material may be made much more readily and easily than the carbolized gauze of Professor Lister. A very thin cheap porous calico free from stiffening and unbleached, is the proper material to use, and it is to be steeped in the following composition:—Carbolic acid crystals, melted, two fluid ounces; castor oil, two fluid ounces; purified resin, sixteen ounces by weight; methylated spirit, forty fluid ounces,—mix. Melt the resin first, and then add one-third of the spirit, first withdrawing the resin from the fire, when mixed add one-third more spirit with the castor oil previously dissolved in it, lastly, add very gradually the acid in the remaining portion of the spirit. The whole must be agitated until the materials are thoroughly incorporated. The cloth steeped in this is to be pressed so as to free it from all superflous liquid, and after being hung up until all smell of spirit has gone kept in tin cases until wanted. (Mr E. Lund, p. 198.)

DISLOCATIONS OF THE HUMERUS.—*Jarvis' Adjuster.*—Jarvis' adjuster is an apparatus by which luxations of the shoulder may be reduced with much more ease and certainty than by any other method. A shoulder collar, something like a miniature horse collar, is fixed on the shoulder, and the arm just above the elbow encircled by a broad firm band. These are the two fixed points, and separation of them can be effected to any extent necessary, and with any degree of power desired, by means of a rack-bar contained in a steel case open at the top. The bar is moved within the case by means of a lever and cog-wheel. This apparatus admits of free movement of the limb whilst the extending force is yet constant and steady, and it will be seen at once what an advantage this must be in cases in which the head of the bone is driven through muscular structures which tightly embrace its neck. (Prof. Stokes, p. 181.)

DISLOCATION OF THE HIP.—*To Reduce Dislocation on the Dorsum Ilii by Manipulation.*—Give chloroform fully, flex the leg on the thigh and the thigh on the abdomen, abduct the limb fully, and then rotate outwards. (Dr. H. C. Cameron, p. 185.)

DRY EARTH AS A DRESSING FOR WOUNDS.—Dry clay, finely powdered and sifted through muslin, or ordinary fine earth, is a dressing of the greatest value in surgical cases. It removes all fetid odour from putrid wounds or cancerous sores. In a case of compound comminuted fracture of the tibia the wound rapidly healed, and the patient was almost

free from pain throughout the case. Ulceration of the leg may be cured by means of the fine clay dressing, without confinement to bed. (Dr. J. Groves, p. 196.)

EXCISION OF THE THYROID GLAND.—The thyroid gland may be excised, provided the enlargement is central and not of too large size. The danger from hemorrhage may be to a great extent done away with by ligaturing the parts before excising. The principal thing to remember is that the cellular sheaths of the vessels entering the gland at the point of entrance leave the vessels and spread over the gland, making a delicate membranous sheath for the gland, and in all ligatures applied this sheath must be included, as the vessels themselves are too fragile. This sheath having been exposed, and the vessels cut in doing so tied, by means of an aneurism-needle the gland must be ligatured in quarters, the ligatures being placed deeply. In one case catgut was used and the ends cut off short. The gland should then be removed by scissors and the wound closed. The removal of the gland is to be very carefully done, there should be no tearing away of the parts. (Dr. P. H. Watson, Edinburgh Med. Journal, Sept., p. 252.)

FRACTURES.—*Iron-wire Box for.*—Dr. McNalty recommends a box made of iron-wire, which folds up when not in use and can be let down at the sides. "Bandages, with the exception of the one for the foot, are rendered unnecessary. The wound can be dressed by letting down the sides, unless it be confined to the back of the limb, and in most cases can be seen without disturbing the apparatus. The foot-rest remains fixed when the sides are let down; this is a most important point, as it is well known that one of the greatest difficulties in the treatment of fractured leg is to prevent either inversion or eversion of the foot. The sides and foot-rest are sufficiently high to keep off the pressure of the bedclothes." (Dr. G. W. McNalty, Lancet, Nov. 8, p. 660.)

JOINTS.—*Mimic Diseases of.*—It is often difficult to distinguish a nervous or neuralgic affection from a real one. "First, as to pain. Alone, it is not to be at all relied on for a sign of inflammation of a joint, especially if it be severe. If a patient be ready to scream when the accused joint is touched, and yet the joint is not over warm and the patient not feverish, you may be nearly sure of neuromimesis; and more nearly still if the pain be rather in the parts outside or about the joint than in the joint itself, so that a light touch is said to hurt as much as a hard one, or a pinching of the skin as much as pressure on the joint itself. You must not even

rely on what are supposed to be characteristic pains, such as those felt at the knee for disease of the hip, or about the middle of the arm for disease of the shoulder, or even those grinding and burning pains at night which some regard as characteristic of ulceration of cartilages. All these may be mimicked." "You may take as a general rule, that acute pain in a joint, if it has existed for even a few days without either local or general increase of temperature, is not a sign of acute inflammation of the joint. It may be rheumatic disease or rheumatic gout, or some slight inflammation after injury, in a neuralgic person, but it is not acute inflammation." But pain often puzzles us. "You may often judge the pain to be mimicry by its inconsistent severity. If with scarcely any other sign of disease a joint will not permit the slightest movement because of pain, you may greatly suspect the reality of disease; but be very careful not to overlook the signs of increased pain on movement in cases of slight real disease, especially in the diseases of the hip in children. You may often find this the only sign making it clear that a child has real hip-disease. A suspected joint may allow free and smooth and painless movement till, for instance, in extreme abduction of the thighs, one adductor becomes much tenser than the other, or in extension the loin is quickly raised, or by some other movement it becomes evident that the joint will not allow extreme movement without pain, although within all but its extreme range its movements may be free and painless. In these, among other cases, chloroform or ether may help in diagnosis. In the real disease, as in the mimic, while the patient is utterly insensible, the joint may be moved as widely as in health, unless indeed there be such changes in its structure as might alone have proved its disease; but commonly you will observe that, in the real disease, the muscles become alert, and restrain the movement of the joint before the patient has regained consciousness; while in the mimic disease there is no restraint till consciousness is completely regained. The test is a delicate one, but I think I may be sure that it is a true one, and fit to be relied on, whenever the chief sign of disease of a joint is a restraint of movement on account of pain and the guarding action of muscles. Closely allied with this pain on movement of an inflamed joint is its stiffness, with contraction or other set posture, dependent on muscular action; for this posture, whether it be due to choice or to reflex movement, is the posture of greatest ease, or of safest guard against weight or shock or other causes of pain. The absence, therefore, of the fixed, or nearly fixed, posture usually observed in a diseased joint, may always

suggest the suspicion of mimicry. It would be rather strange to see a hip or a knee extended after many weeks of such pain as would be felt in an acute arthritis, unless, indeed, they were rheumatic or gouty, with exaggerated pain, or had been carefully maintained in good position. It would require the presence of many other signs of real disease to counterbalance the absence of this sign; for diseased joints, left to themselves, will be habitually, or always, in the position easiest to the patient. But the reverse of this is not true. Very commonly a joint mimicking disease assumes the posture of disease—assumes and maintains it stiffly in even an extreme degree. This may be seen even when there is no objective pain in the joint; but much more, when the joint is a little really painful, as after a blow or strain, the nervous condition of a patient may either make this pain so intense as to demand the position of greatest ease, or may bring about this position for the relief of even a little pain. Especially the posture of hip-disease is apt to be imitated by the drawing up of one side of the pelvis and rotating it, so that the limb looks shortened." (Sir James Paget, p. 167.)

THE ELASTIC LIGATURE.—Professor Dittel, of Vienna, accidentally discovered that the slight yet continuous pressure of a simple elastic thread causes absorption of the parts pressed upon, so that the thread eats its way into the structures, and this without pain. A girl died with severe brain symptoms, and it was found that the india-rubber band of her hair-net, which she had worn night and day, had ulcerated through the whole thickness of the calvarium, and set up meningitis. Professor Dittel has now performed a large number of operations by means of the elastic thread or band, including five amputations of limbs. The time required for the separation of a ligatured part varies with the amount and density of the tissues which have to be divided; *e.g.*, for the separation of the mamma, from eight to twelve days are required. Among the operations for which the elastic ligature is admirably adapted may be named fistula in ano. (Sir H. Thompson, British Medical Journal, Nov. 29, p. 631.)

AFFECTIONS OF THE SKIN, &c

ALOPŒCIA.—Cut the few thin hairs on the bared spot as short as possible, then thoroughly cleanse the skin from scruff by washings in warm water without soap. Then let the skin be rubbed for half an hour at a time with a drachm of tincture of phosphorus to the ounce of castor oil. This should be

done three times weekly and persevered in for six months. The hair will usually soon begin to grow. (Mr. H. Finch, p. 301.)

CATGUT LIGATURES.—Catgut is an admirable material for ligatures, especially for wounds in which there is reason to expect union by the first intention, and in which it is desirable to avoid any mark, as in wounds of the face. Catgut never causes irritation, nor gives rise to suppuration as does silk, nor like metallic sutures does it give much pain. It is also very readily removed, not tending to tear open a wound during its withdrawal. (Dr. J. C. O. Will, p. 297.)

RINGWORM.—*Oleate of Mercury.*—All that is necessary is to rub gently into the spot or spots a few drops of oleate of mercury (10 per cent. strength), and repeat this as necessary. The oleate of mercury possesses many advantages over other remedies. It is both certain in action and produces no staining or injury of the skin. It is also painless in its application. It readily penetrates into the sebaceous glands, hair follicles, and even into the hairs themselves, the mercury being in a state of solution in an oily medium, and it is therefore much more likely to destroy the fungus than the spirituous or aqueous solutions of mercury, &c. This penetrating power of the oleate may be increased by adding a small quantity of ether (one part to eight) to it. (Mr. L. Cane, p. 286.)

SPINA BIFIDA.—*Injection of Iodine.*—Dr. Watt, of Ayr, relates a case of spina bifida thus treated. Three times a small quantity of fluid was removed from the tumour, a week intervening between each operation. At the third operation, half a drachm was injected of a solution containing ten grains of iodine and thirty grains of iodide of potassium in an ounce of glycerine. Nine days afterwards the tumour is reported as being slightly decreased in size. Four weeks from the time of operation the tumour was nearly level with the back, and was replaced by a large half-dried resistant scab. At present a cake of firm condensed tissue occupies the site of the scab, and the child is in good health. This plan is that originally introduced by Dr. James Morton. (Dr. J. R. Watt, p. 299.)

ULCERS OF THE LEG.—Prepare an ointment consisting of three pounds of finely powdered chalk to two pounds of lard. The lard should be melted, and the chalk added gradually and stirred in. By the use of this ointment to a sore on the leg a crust is formed, first on the surrounding skin, then on

the margins, and finally on the surface of the ulcer. It should be spread on lint, and the leg bandaged evenly with domette flannel. The chalk combines with the discharge from the sore, neutralises its acridity, and forms an artificial scab, under which the healing process goes on rapidly. Suppose a sore is always covered with a thin greenish slough, steep a pledget of "chloralum wool" in equal parts of water and the solution of chlorinated soda (liq. sodæ chloratæ, B.P.); fill up the cavity of the ulcer with the wool, lightly teased out, and cover it with a chalk ointment dressing. Chlorine is much superior to carbolic acid in its cleansing power. (Mr. J. K. Spender, p. 291.)

The sore having been washed clean by the patient, saturate it with a weak solution of carbolic or sulphurous acid. Take some pieces of stout empl. saponis, two inches broad, and long enough to overlap four inches after passing completely round the limb. Pass them one by one through hot water containing a little carbolic acid, and apply them neatly round the limb, from three inches below the diseased surface to about the same distance above. The limb is then to be carefully bandaged from the toes to the knee. Change the strapping about once a week. (Dr. J. G. Black, p. 288.)

AFFECTIONS OF THE EYE.

AMAUROSIS.—*Strychnia.*—Stychnia administered by the mouth produces quite the same results as when injected hypodermically, but is perhaps not quite so rapid in its action. It is therefore not worth while to give it hypodermically. To obtain all the good which strychnia can produce, it is necessary to keep the system under the full physiological effects of the remedy by administering a dose as large as the patient can bear comfortably. As a rule half a grain in divided doses can be borne by most persons. The intense bitterness of taste may be avoided by giving it in the form of sugar-coated granules of $\frac{1}{30}$, $\frac{1}{20}$, $\frac{1}{15}$, $\frac{1}{10}$ of a grain in each, commencing with the smaller doses. (Dr. J. J. Chisolm, p. 272.)

CATARACT.—*New Method of Extracting.*—The following is the plan of operating pursued by Dr. Taylor. After making the corneal section, excise a minute piece of the outer edge of the iris, and separate it with very fine scissors from its marginal attachments to an extent sufficient to permit of the easy exit of the lens through the wound. Lacerate the capsule through the slit in the iris, and extract the lens in the usual way. By this operation the advantages of iridec-

tomy as pursued by Von Gräefe are obtained, without the dazzling and defective vision occasioned by the sacrifice of a portion of iris. (Dr. C. B. Taylor, p. 270.)

IRITIS.—Upon nothing can we so much rely as on the rapid dilatation of the pupil by belladonna or atropine. The earlier it is applied the better it will act, and its action places the iris at rest and out of harm's way from the effusion of coagulable lymph within the pupil, or between the iris and the lens. Should the eye prove intolerant of atropine, a leech should be applied just below the lachrymal tubercle. It will then generally be found that the eye tolerates its action. During the night, extract of belladonna, rubbed up with a few drops of glycerine, should be freely applied to the brow, especially as at night the disease often makes great progress. The belladonna should be spread on oiled silk to prevent it drying up. Spirit should never be used in the preparation of solutions of sulphate of atropine, as it is unnecessary, and liable to produce great irritation. (Mr. J. Hogg, p. 274.)

SLITTING THE PUNCTUM LACRYMALE AND CANALICULUS.—A small instrument invented by Mr. Greenslade gives great facilities for this operation. A director and knife are combined. The director is passed, and then the knife, which was concealed in the handle, is slid along it. The advantages are that one hand is at liberty to steady the eyelid, and that all danger of wounding other structures than those required to be divided is avoided. (Mr Greenslade, p. 272.)

MIDWIFERY, ETC.

CANCER OF THE UTERUS.—Our best means of treating cancer of the uterus are the administration of ergot internally and the application of caustics. The ergot should be given twice a day for several months. It possesses the power of producing uterine atrophy. The organ is kept contracting, and the blood supply is diminished. The use of caustics should be combined with this. The best, that is, the most destructive, and at the same time most manageable, are the chloride of zinc, the dried sulphate of zinc, and the nitrate of copper. The dried sulphate of zinc should be applied in considerable quantity, so as to produce a slough, the vagina being protected by cotton wadding, tipped at the uterine end with olive oil. The nitrate of copper is most suitable for injection a little way into the uterus. A healthy healing sore may not unfrequently be obtained by these means. (Dr. A. Milne, Edin. Med. Journal, May, p. 1037.)

EXCISION OF INTERNAL HEMORRHOIDS IN THE FEMALE.—Excision of internal hemorrhoids is preferable in every respect to ligation, yet is attended with great risks from hemorrhage. In the female, however, the lower part of the rectum may be everted from the vagina, and the parts seen, and thus any hemorrhage can be at once arrested by the application of some astringent or caustic. (Dr. H. R. Storer, p. 367.)

FEMALE RECTUM.—*An Improved Method of Examining.*—The anus and lower part of the rectum of the female may be completely everted by pressure from within the vagina, The patient being upon her side, introduce the finger into the vagina a sufficient distance, and then press steadily backwards and downwards upon the sphincter ani. If there is spasmodic contraction of this muscle, it should be ruptured by rapid dilatation with the thumbs, as suggested by Recamier for the treatment of anal fissure, (Dr. H. R. Storer, p. 367.)

Visual Exploration of the Upper Rectum.—First forcibly and rapidly dilate the sphincter ani with the two thumbs, so as to paralyse or rupture it; then introduce a vaginal bivalve speculum. It will be found that the visual exploration of the upper rectum is perfectly practicable, and to a much greater extent than can be accomplished by the endoscope. (Dr. H. R. Storer, p. 367.)

Duck-Bill Speculum for the Rectum.—A small duck-bill speculum is infinitely superior to the common speculum ani in affording light and space in operations on the rectum. If necessary, the sphincter ani and rectum may be slit towards the coccyx with a bistoury, to afford more room. (Dr. Demarquay, p. 370.)

INDUCTION OF PREMATURE LABOUR BY ELECTRICITY.—The induction of premature labour may be easily and safely effected in the following manner:—Pass within the os a metallic sound covered, except at the point, with a non-conducting material; this is made to form one pole of a galvanic machine, the other being placed upon the abdomen. A current of electricity, continuous or interrupted, is to be passed through the parts for about ten minutes, and the process repeated daily until the expulsive action of the uterus is induced. A little dilatation of the os is generally the first effect, and it is well then to substitute a larger conical point. Gaiffe's battery is so exceedingly portable that it is very suitable for medical purposes. (Mr. J. Varley, Dr. B. R. Morris, p. 314.)

INERTIA OF THE UTERUS.—*Electricity.*—There are cases in which the uterus having acted for some hours, and the labour being advanced to a greater or less extent, the pains cease and cannot be brought on again by ergot, friction, or anything else. In such a case the passage of a current of electricity brought on a moderate pain, the first for eleven hours, and the pain continued to increase until the current was interrupted. The head descended to the brim. After waiting about ten minutes the current was re-applied for three minutes, which had the effect of bringing the head into the brim. By the next pain the head was born, exactly twenty-three minutes from the commencement of the application. Maw's self-acting electro-magnetic machine is the best for use in obstetric practice, as it is difficult to get a female attendant to work a machine steadily. One of the electrodes should be placed on the abdomen, and the other on the perineum. The best abdominal electrode is a flat piece of metal, the size of the two hands opened, and curved to fit the transverse abdominal curve. This is to be retained by the patient's hands, a layer of blanket being interposed. (Dr. A. E. M'Rae, p. 326.)

Mr. Storrs, of Doncsster, relates a case in which injection of perchloride of iron, external pressure, kneading the uterus, cold affusion externally, and injection of cold water per vaginam produced no effect. A Stöhrer's portable coil machine was procured, and one pole placed on the abdominal walls and the other within the cervix uteri. Contraction at once ensued, and complete arrest of the hemorrhage. (Mr. R. Storrs, p. 353.)

INTERSTITIAL FIBROUS TUMOURS OF THE UTERUS.—It is of the utmost importance to determine whether any given interstitial fibrous tumour is nearer the mucous or the peritoneal coat of the uterus, because the prospect of successful treatment is far greater in the first than in the second case. If the size of the uterine cavity, as measured by the sound, bears a fair proportion to the size of the tumour, we may expect to find the tumour nearer the mucous surface, and consequently the case would be a promising one for enucleation. (Dr. A. Meadows, p. 390.)

It has long been known that if the capsule of a fibrous tumour is divided, the growth is gradually forced through the aperture by the contraction of the uterine fibres expanded over the rest of its surface. Dr. Greenhalgh, in a case of this kind, forced a small conically bulbous cautery into the tumour, behind the posterior lip of the os uteri, and through the upper wall of the vagina. In a week the opening thus

made had enlarged to the size of a crown piece, and through it the tumour was beginning to protrude. In four or five days more the mass came to occupy the whole vaginal cavity, and was partly removed by the écraseur. Ultimately the entire mass was turned out by the finger, and a good recovery made. (Dr. A. Meadows, p. 393.)

LABOUR WITH CONTRACTED PELVIS.—It is exceedingly doubtful if the operation of inducing premature labour ought ever to be employed in cases of contracted pelvis. When the labour actually comes on, we ought first to see what nature is likely to accomplish unaided, without allowing the mother to run unnecessary risk. If artificial aid is found to be necessary, the long forceps should then be applied, and if delivery is found to be impossible it must be effected by cephalotripsy or craniotomy. (Dr. A. Macdonald, p. 316.)

NEW MIDWIFERY FORCEPS.—Dr. Lowenthal, of New York, describes a midwifery forceps used by him, which possesses some peculiarities worthy of note. Like those described by ourselves (Eds. of *Retrospect*) in 1869, the principle of it is the passage of the two blades, as one, into the hollow of the sacrum. For this purpose the convexity of one blade fits into the concavity of the other. When introduced the blades are separated, and are glided round the head of the child in opposite directions. The difference between the two instruments is that, whereas in our instrument the blades and handles are completely separable, in this instrument they are joined at the lock, which at the same time is so constructed as to admit of the blades revolving round the child's head. (Dr. Lowenthal, New York Medical Record, Feb. 15, p. 94.)

ORIFICES OF THE UNIMPREGNATED UTERUS.—The os externum is surrounded by circular muscular fibres which perform the part of a sphincter, and the same is the case with the os internum. These sphincters open for menstruation, probably for conception; and the internal one closes on the contact of a foreign body, such as the uterine sound. The point of this instrument is generally arrested when it reaches the os internum. Sometimes a slight pressure will pass it into the uterine cavity, sometimes it will not, and nothing short of unjustifiable force will succeed; although in these same cases a small wax bougie, warmed and bent, will generally pass. Resistance to the sound at the os internum is a sign of a healthy natural vital state of the parts. It is necessary to establish these facts, as otherwise any healthy woman may be pronounced pathologically wrong, and treated for strictures which do not exist. (Dr. J. H. Bennet, p. 302.)

OVARIAN DROPSY.—*Diagnosis.*—It is generally supposed that there is no more conclusive diagnostic test of ovarian dropsy than the linseed-tea looking liquid which may be drawn off by an exploratory puncture. Mr. Hutchinson, however, relates a case in which this proved fallacious, and which turned out to be extra-uterine gestation. The physical characters of the disease exactly resembled those of ovarian pregnancy, except that both loins were resonant. In ovarian dropsy one loin is dull on percussion. To avoid such an error in future, Mr. Hutchinson suggests that the patient should be made to support herself on her hands and knees, when the fœtus, if present, will fall to the lowest part and can be recognised by the surgeon as a heavy solid body. (Mr. J. Hutchinson, p. 354.)

PERSISTENCE OF THE HYMEN.—A number of cases are related by Dr. Gray, of Glasgow, which prove that persistence of the hymen is compatible with the wedded state, that destruction of it does not necessarily follow even the calling of a prostitute, and hence, that its persistence after attempted rape must not be relied upon solely as evidence sufficient to disprove the charge. (Dr. J. St. C. Gray, p. 395.)

POST-PARTUM HEMORRHAGE.—*Anticipation of.*—When the pains during labour are strong and quick, ceasing almost suddenly, and the intervals between them long in proportion to the length of the pains, post-partum hemorrhage is almost sure to occur. The uterus in these cases contracts sharply, and then becomes fully relaxed, and the same tendency continues after the birth of the child. Being able in this way to foretell the occurrence of flooding, it becomes easy to prevent it by giving ergot at the proper period before the birth of the child, so as to alter the character of the pains and make them longer, and the intervals between them shorter. (Dr. E. Whittle, p. 342.)

Injection of Perchloride of Iron.—There *are* cases in which the hemorrhage after removal of the placenta cannot be arrested by either pressure or cold, nor yet by the administration of ergot. It is in these cases that the injection of a solution of perchloride of iron should be tried. Six ounces to the pint of water was the strength of the solution used in a case related by Dr. Thomas Chambers. The effect was not produced in the way we should have imagined, it was not to cause coagulation of the blood at the orifices of the uterine veins, the uterus being not firmly contracted, but to cause contraction of the organ and so arrest the hemorrhage in the natural manner. (Dr. T. Chambers, p. 347.)

The result of experience in the use of perchloride of iron injections is very favourable. Instead of being obliged to grasp the uterus for an hour or two, and feeling afraid to leave the patient perhaps for many hours, the use of the iron solution at once removes all doubt and difficulty, by inducing firm and permanent contraction. Before injecting the fluid into the uterus, all coagula or remaining portions of placenta structure must be carefully removed. (Dr. Steele, p. 349.)

PUERPERAL CONVULSIONS. — *Chloral.* — The introduction of chloral appears to have quite revolutionized the treatment of puerperal convulsions. Whereas previously we had nothing worth relying upon, we now have an agent which, if we can once get it introduced in the proper dose into the circulation, is almost certain to prevent a recurrence of the attacks. A very interesting case is related. (Dr. A. D. Mackay, p. 341.)

Enemata of Chloral.—In a case of puerperal convulsions in which no remedy could be given by the mouth, a drachm of chloral hydrate was given in two enemata. After the second no more convulsions occurred, and quiet sleep followed. (Dr. J. Barclay, p. 340.)

RUPTURE OF THE PERINEUM. — When the perineum is ruptured into the rectum, take an ordinary good large curved needle and pass it in deeply on each side of the ruptured perineum, going well down to the bowel, or even into it. Use a good piece of thick wire, twist the ends together over the front, and get the parts drawn well together. These sutures can be left in much longer than quilled ones, and with less damage to the skin and surrounding parts. Then pass the finger into the rectum, and with a straight, blunt-pointed bistoury divide the sphincter on both sides to ensure freedom from movement of the torn edges of the wound. (Mr. C. Heath, p. 364.)

SUPPOSED STRICTURE OF THE CERVIX UTERI.—The treatment of dysmenorrhœa arising from contraction or stricture of the internal os is both unscientific and unsuccessful. The stricture in most cases is merely a congestive swelling of the lining membrane of the part, and although temporarily relieved by the hemorrhage produced, rapidly returns, and any relief obtained is merely temporary. Quite as good results and better in the long run, are obtained by scarification of the os and the passage of graduated bougies. By scarification, congestion of the lining membrane of the womb and of the ovaries is relieved, and the passages are relaxed. By this means the bougie is more easily introduced and does not rouse or increase irritation of the canal and interior of the uterus. (Dr. W. Cumming, p. 305.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON DIGITALIS IN ACUTE FEBRILE DISEASES.

By Dr. ANSTIE.

[In this paper Dr. Anstie invites our attention to the effect of this drug in cases of fever.]

The special subject which will now be discussed is suggested to us by the appearance of a pamphlet by Dr. T. W. Grimshaw on the action of digitalis on the weak heart of typhus; a paper which contains some remarkable facts, although I am not able to agree with all its conclusions. The question of the power of digitalis as a heart-tonic in the so-called adynamic fevers generally—not merely in typhus—is becoming very important: and the following remarks will be concerned with this alone, to the exclusion of other questions, such as the power of digitalis to lower temperature in acute disease.

I can hardly preface my own observations more effectively than by bringing forward some of Dr. Grimshaw's facts. When it is remembered how strikingly the pulse of typhus fever exhibits the characters of low arterial tension and low heart-force, it must indeed be astonishing, to those who cherish any remnant of the classical belief in digitalis as a heart-depressor, to find Dr. Grimshaw giving 1 ounce, and even 1½ ounce of the *infusion* of digitalis every three hours for five or six days together, not only with impunity, but with seeming benefit. Whether the benefit was real or not, at least this is certain, that in nearly all the cases there was a total absence of those symptoms of heart-depression which used to be held up as a bugbear of digitalis treatment; indeed, on the contrary, it is asserted by Dr. Grimshaw, with apparent accuracy, that both the heart-force and the arterial tone were markedly invigorated. And in addition to this, it is to be remarked that but small amounts of alcoholic stimulants were given in any case, and in several no alcohol was administered. There were but two

deaths out of twelve cases, and in the whole of the ten recoveries the patients were adults.

I do not propose to dwell any further on the cases brought forward by Dr. Grimshaw, which have been cited simply as proof of the fact that in typhus, even of severe type, the phenomena of cardiac and vascular depression are relieved rather than aggravated by the use of digitalis in very large doses. Observe, that these cases, in which the *infusion* of digitalis was employed, stand on a very different footing from experiments in which large doses of the *tincture* had been employed: so long as the evidence consisted only of instances in which the latter preparation had been used it was impossible to say how far the result had been influenced by the large quantities of alcohol necessarily given. In Dr. Grimshaw's cases, on the contrary, we have to deal with an uncomplicated digitalis action.

It would be a mistake, however, to limit our consideration of digitalis as a heart-tonic in febrile diseases to the case of typhus. It must not be forgotten that the latter is an affection that runs a brief course, and in which asthenia is not the mode in which death usually occurs. Some of the best authorities on fevers have remarked that the issue in uncomplicated typhus depends upon the intensity with which the nervous system is affected, far more than on cardiac asthenia of local origin; and certainly the characteristic aspect of such cases is pre-eminently that of overwhelming nervous poisoning. No doubt the muscular tissue of the heart is frequently, if not always, considerably changed, and both Murchison and Buchanan state, from personal observation, that in fatal cases this affection is not a mere fatty softening, but sometimes, at least, involves more or less elaborate changes of the same kind as those which have been so carefully described by Zenker as occurring in the heart and voluntary muscles of typhoid patients. But on the whole it seems evident that local cardiac changes count for much less in a brief disease like typhus than in a prolonged affection like that form of typhoid in which death occurs from simple heart-failure at an advanced period, and in which "Zenkerism" of the cardiac muscular substance is especially pronounced. It is in connection with this latter form of fever that I have more especially studied the question of weak heart in febrile disease; and before speaking of digitalis as a remedy, I wish to make some remarks upon the clinical facts of the case.

I doubt whether any published account of typhoid fever gives a just idea of the frequency with which sudden failure of the heart—and this alone—is the cause of death in cases of typhoid which run a longer course than four weeks. I do not mean that all these deaths are instantaneous; but the following is a fair average description of what happens:—Either from the

original intensity of the febrile attack, or from the existence of severe complications, the heart has been kept pulsating at a rate varying from 120 to 150 or more, under an internal temperature which was always very high, even at the daily minimum, for some four or five weeks. The inflammatory complications, or the intestinal ulceration, however, have fairly subsided, and the pulse now begins to change. But instead of the simultaneous diminution of frequency and increase of arterial tone which would show the passage to convalescence, we remark (if we observe with attention) a very different kind of pulse-change. Careless feeling of the radial pulse might lead one to believe that the pulse, though somewhat irregular, had diminished very greatly in frequency; but a thoroughly skilled finger, or still more surely a sphygmograph, would immediately bring out the fact that many small intermediate pulsations had been overlooked; in fact, that the pulse is scarcely, if at all, less rapid than before, but now presents the dangerous quality of varying in force from moment to moment, *and that in an unrhythmical manner*. Nothing can be of worse augury than this—more especially when the sphygmograph shows the combination of rounded apices of the individual pulse-waves with unrhythmical undulation of the general line of the tracing. It is under such circumstances that patients often "sink out" after all active mischief would seem to have ceased. It is very noticeable that such a catastrophe is far more common in adults than in children: the hearts of the latter possess a prodigious power of recuperation, which seems to be reduced to a minimum by the time that the middle age—so dangerous to typhoid patients—is reached. At the latter epoch, also, not merely is heart-failure a too likely occurrence in the later weeks, but this form of death may overtake the patient at an earlier date; and there seems reason to believe that in such persons the degenerative process goes on with far less interruption from recuperative efforts than in the young.

It is now well known that a very marked rise in the mortality from typhoid is found in the quinquennium immediately succeeding the development of puberty: and from personal observations I believe that, even so early in life as this, there is a change in the nutrition of the cardiac muscular structure which renders it more incapable of resisting the influence of high temperature and very rapid pulsation for prolonged periods, and which consequently allows, more frequently, the occurrence of death from heart-failure. But the tendency is, doubtless, greatly increased later in life.

I think we are entitled to view the occurrence of muscular heart-failure in acute febrile disease as essentially dependent upon an enforced rapid action, under high temperature, pro-

longed for a period which is excessive, *in proportion to the vital recuperative power of the cardiac muscular tissue.* In this point of view we should logically place on a line with the effects of the mere prolongation of high temperature and rapid pulsation, in protracted typhoid, the effects of a shorter exposure, to these conditions, of a heart whose tissues had been previously so modified by pathological degeneration as to render their restoration to a healthy state unusually difficult. This principle is very important. We should expect heart-failure to be a very formidable danger in delirium tremens, with high temperature and rapid pulse, if the patient were an old drinker; and, similarly, we should expect that acute diseases, like pneumonia or erysipelas, occurring in persons who had long been addicted to alcoholic excess, would be very liable to terminate fatally in the same way. Nothing can be more certain than that this liability exists; it is the terror of every experienced physician who encounters these acute maladies in old topers. Such persons cannot endure for more than a very limited period the presence of pyrexia and rapid pulsation; their hearts give way under the combined pressure.

In applying considerations of this kind to the question of digitalis treatment for heart-weakness in febrile disease, it is at least evident that there are very good *primâ facie* grounds for expecting valuable results in a considerable proportion of cases. By a strange perversity, which is the more surprising in view of the greatly improved pathological knowledge of the present day, too many teachers of medicine ignore the influence (which cannot be too highly estimated) of the previous condition of the bodily organs of any given patient upon the cause of acute febrile diseases by which he may chance to be attacked. Such an error is to be strenuously avoided by any practitioner who desires to attain a mastery of treatment: and certainly, in no instance should it be more carefully shunned than in the use of digitalis in acute febrile disease.

What is it, in fact, that we expect to gain from digitalis in acute febrile disease? Putting aside the question of reduction of temperature, which we are not now considering, we may endeavour to operate on the heart itself. It matters not whether we adopt the theory of vagus stimulation or that of direct tonic action on the muscular fibre *plus* a stimulation of the vasomotor system, producing tonic contraction of the arterioles; the practical effect at which we aim (and are justified in so aiming by a number of experimental facts) is twofold—the simultaneous slowing and strengthening of the ventricular contractions. It may be asked whether this in itself amounts to any real advantage, unless the course of the disease were also cut short. I believe that, in regard to the danger of heart-failure it may be

of great advantage. The normal tendency of muscle which is persistently overworked in proportion to its nutrition is undoubtedly towards degeneration; and, certainly, there can be no more decided instance of such overwork than that of the cardiac muscles when they are forced to contract some 130 or 140 times per minute, for a long time together, under a temperature of 103° to 105° or 106° Fahr. Merely to reduce the frequency of these contractions must surely be an important object, for the sake of preserving the soundness of the muscular tissue.

A question of much interest arises whether the impunity with which digitalis has been prescribed in such large doses by Dr. Grimshaw is due to the fact that the infusion of digitalis is a weaker preparation than British physicians have been accustomed to suppose. Upon this point there is a curious conflict of evidence, which has never yet been cleared up. The researches of modern chemists (especially those of Wiggers and of Nativelle) have made it clear that digitalis contains two active ingredients—digitaline and digitaleine,—of which the former is said to be decidedly the most powerful of the two; but of these only the latter is soluble in water to any considerable extent, while the more powerful principle requires alcohol for its extraction. On the other hand, there is abundant recorded evidence of very powerful poisonous effects being produced by aqueous infusions and decoctions of digitalis. In making comparisons between the potency of the infusion and that of those preparations of digitalis which contain the more powerful alkaloid, it is necessary, as already said, to leave the tincture out of the question, since the alcohol in that preparation confuses any experiments which are made, and renders their results altogether equivocal. It remains, then, to inquire whether the powder of the leaves, the alcoholic extract, and the crystalline digitaline itself, can be given in respectively large doses in the condition of a weak heart in febrile diseases. Now, it is remarkable that the so-called "digitaline" of Homolle and Quevenne (a mixture, in fact, of digitaline with a certain amount of digitaleine and digitalose), the dose of which, as recommended by the inventors, is from $\frac{1}{17}$ to $\frac{1}{11}$ grain, has been given by Trousseau, in uterine hemorrhage, in as large a quantity as 1 gramme (15·6 grains) in the twenty-four hours; and by many other physicians, in pneumonia, typhoid, &c., to the amount of nine or ten grains daily. And the powdered leaves of digitalis have equally been given, especially by the German physicians, in doses which sound perfectly frightful as compared with the doses laid down by authorities in this country. On the whole, then, it appears at least very probable that there are some circumstances which confer on the organism a large degree of tole-

rance for digitalis; and, looking to the remarkable experience of Trousseau above mentioned, we get more than a suspicion of the grounds of this tolerance. Trousseau gave as much as 15·6 grains of digitaline in twenty-four hours to a woman suffering from uterine hemorrhage; and we can hardly doubt that in a healthy person a much smaller dose than this would have caused fatal tetanus of the heart. Good digitaline of Homolle and Quevenne is probably quite fifty times (not 100 times, as the inventors say) as strong as fresh digitalis leaves; and, not to mention the instances—due, probably, to idiosyncrasy—in which as small a quantity as one or two milligrammes (·0156 grain, or ·0312 grain) has produced uncomfortable symptoms, it has very frequently happened that doses twice or three times as large as this have caused nausea and great depression. The true explanation of the tolerance of such enormously greater doses in cases of uterine hemorrhage must be sought in the modern physiological experiments which have shown that the real action of digitalis on the heart is that of a stimulator, instead of a paralyser, of the cardiac muscular substance; in accordance with which fact one may well suppose that the extremely weakened cardiac muscular fibre of a patient, exhausted by profuse hemorrhage would sustain and even require a dose of digitalis, merely to support a sufficient amount of contraction to continue life, which in health would fatally tetanise the heart.

The general effect of the various considerations which have now been advanced seems to be this:—that the action of digitalis (*a*) as a sustainer of exhausted heart-power, and (*b*) as a restrainer of the rapidity of pulsation, seems to especially point it out as a promising remedy in that very large class of acute febrile diseases in which the greatest danger is failure of the heart before the malady has had time to run through its natural periods of development; and that there are strong grounds for holding this opinion, whether we admit or deny the reduction of temperature which is so strongly maintained by many eminent authorities. On the latter point I cannot but think that judgment should be as yet suspended; not that I deny the fact that febrile temperature can be reduced by large doses of digitalis, but that a careful review of the literature of the subject leaves me in doubt whether the lowering of temperature is not, in an inconveniently large number of cases, attended with an amount of nausea and depression which more than counterbalances the advantage of the mere cooling. In my own experience, which may have been unfortunate, it has been difficult to frame any rules of dosage and circumstances under which one might with confidence expect a beneficial reduction of febrile temperature. But I can answer for the fact that,

even without this action, digitalis has, to all appearances, proved of the utmost value in many cases of acute disease running a protracted course or occurring in subjects whose tissues were damaged by disease or intemperance. And the more I reflect upon the matter, the more I am inclined to trace these benefits to the sustaining action of digitalis upon the enfeebled heart.—*Practitioner, Sept.* 1873, *p.* 179.

2.—ON EPIDEMIC TYPHOID RESULTING FROM AN INTERMITTENT WATER-SUPPLY.

By the EDITOR OF THE LANCET.

It will be remembered that in May last Dr. Blaxall, one of Mr. Simon's staff, was sent to prosecute a sanitary inquiry at Sherborne, in consequence of the prevalence of both endemic and epidemic typhoid. His report, now published, on enteric fever in the town of Sherborne, is unusually interesting, as it shows how clearly the chief epidemics of enteric fever may be traced to contamination of the drinking water by sewer air, and fully bears out the views recently ventilated by Dr. Carpenter as to the dangers of an intermitting water-supply. Dr. Blaxall bases his opinion, that the epidemic this spring was caused by persons drinking of water contaminated with sewer air, on the following considerations:—First, the sudden appearance of seventy-three fresh cases in the first week of March (the average weekly number for the three previous months having been two and a quarter)—viz., after the Board of Health water had been shut off every night during February; second, the gradual diminution of cases after the first week in March, during which month the water was not turned off at nights; and, third, the manner in which the cases were scattered about the town; but, mainly, the far greater proportion of cases occurring amongst persons supplied with the Board of Health water than amongst those who derived their water from other sources. The danger of an intermitting water-supply here exemplified is so important, and yet so little appreciated, that we consider the following details, taken from the above-mentioned report, will well repay a careful perusal.

Sherborne, according to the last census, had a population of 6041. In 1867 it was visited by an epidemic of enteric fever, which caused six deaths; since then enteric fever has continued endemic, doubtless owing to the defective means for the removal of excreta, and to the water having been contaminated from time to time by sewage or sewer air. The accompanying table shows the progress of the recent epidemic, which caused

13 deaths in the first four months of this year, and aids much in elucidating its cause.

Number of New Cases of Enteric Fever.

In the month of	December, 1872	2
,,	January, 1873.....	12
,,	February, 1873	15
In the week ending	March 8th	73
,,	March 15th	45
,,	March 22nd	28
,,	March 29th ...	17
,,	April 5th.....	17
,,	April 12th	23
,,	April 19th	2

It will be observed that rather numerous cases occured in January and February; that in the first week of March there was a sudden and great increase, and during the remainder of the month a gradual diminution.

Dr. Blaxall made a careful inquiry into all the conditions likely to affect the health of the community, particularly with respect to the supply of articles of general consumption. The influence of milk in the distribution could be excluded, but respecting the circumstances of the water-supply the following facts were ascertained. During December and January last the Board of Health water was frequently shut off at a point slightly above the houses on the highest level of the town, and the same thing was done during every night in February. It is known that when the water was thus shut off there were certain delivery-pipes up which a rush of air occurred immediately the tap was unscrewed. Now in many instances the pipes open with taps into watercloset pans, there being a frequent absence of service-cisterns. At night, after the closet had been used, the tap would be turned on for the admission of water; none flowing, the tap would frequently not be turned off again, thus a direct passage into the water mains would be left patent. Even this accidental leaving of the tap open was not necessary, as many taps, being broken, permitted a continuous flow of water during the day, but at night, no water being present, were uninterruptedly free to admit sewer air. Through these openings, when water did not flow, air commonly passed from the closet-pan to the water-pipe, the system of pipes for the water-supply becoming, in fact, the ventilating channels for the closet-pans; and, if a pan happened to be full, its contents might even be sucked into the water-pipe. In January and February, when at least twenty-seven cases of enteric fever were in the town, closet-pans thus circumstanced were doubtless in some instances tainted with

the specific contagium; some of them may even have been filled with the excreta of fever patients. The sewer air which entered the pipes at night, when the water was turned off, would be driven forward in the morning, when the water, turned on again after washing the pipe-surfaces upon which foul air had been resting for hours, could hardly (even supposing no actual excrement to have got into the pipes) have failed to contain fever contagium; and this water was destined to be drunk by the people.

The part played by the Board of Health water in the recent epidemic is tolerably evident from the adjoining tabular statement:—

Houses in Sherborne, 1010	780 supplied with Board of Health water; 148, or 19 per cent., visited by fever. 230 supplied with other water; 13, or 5·6 per cent., visited by fever.
Persons attacked between Dec. 1 and May 10	In the 780 houses, 226; or 29 cases per 100 houses. In the 230 houses, 17; or 7·4 ditto.
Persons attacked in the first fortnight of March	In the 780 houses, 111; or 14·2 cases per 100 houses. In the 230 houses, 7; or 3 ditto.

Though the origin of the Sherborne epidemic, which is so evident from the above history, must be far from an uncommon element in the production of urban typhoid, yet we believe Dr. Blaxall has been the first to demonstrate it in practice; and, though almost despairing, we are encouraged to hope that this danger from an intermitting water-supply may startle the metropolitan authorities into providing that flood which Sir Wm. Fergusson calculates would threaten to run the Thames dry.

Whether or not our ideal sanitary reform—a constant water-supply for London—will result from this fresh knowledge of the danger its neglect entails, we must ever be grateful to those who have added to our sanitary science by their careful deductions from practice and theory.—*Lancet, Aug.* 30, 1873, *p.* 304.

3.—ON SEWAGE AND DISEASE.

By the Editor of the Medical Times and Gazette.

The discovery of the cause of the outbreak of enteric fever in Marylebone has naturally led to much public discussion as to the mode in which the poisonous material has been conveyed into the milk. In our article on the propagation of zymotic disease by milk, last week, we discussed two methods—one a tolerably certain one, the other a probably possible one—in

which the contagium of disease may be conveyed into milk. The certain mode is by polluted water, the more doubtful by polluted air. But the outbreak has led to the publication of speculations as to the mode of infection of milk, to which the actual facts of the case give not the slightest confirmation, but which, nevertheless, in the very imperfect state of our knowledge of the precise methods in which diseases are propagated, may be allowed to demand examination.

On Saturday last a well-known member of our profession—Mr. Alfred Smee, F.R.S.—published a letter in the *Times* newspaper, in the first sentence of which he announced that he is "in a position to explain the occurrence of typhoid fever from the use of milk." The subsequent part of his communication, however, fails to corroborate the opening sentence; for the only fact which Mr. Smee adduces as capable of throwing light upon the occurrence is that some cows of his own, fed on two occasions with sewage grass, produced a milk which, after keeping for twenty-four hours, had a slightly rancid odour, and a butter of an offensive character, which turned rancid a day or two after it was made. Mr. Smee, however, does not add that any one who partook of this milk or this butter was made ill in the slightest degree by it. Now, in the first place, we are informed on the best authority that the pastures of the farm from which the infected milk was exported were not irrigated by sewage, nor in any other mode was sewage manure specially employed. We cannot, therefore, but think it a great pity, so far as this particular outbreak of fever is concerned, that a theory should have been started which might have had the effect of diverting the attention of the scientific public from the real source of the mischief. The account which Professor Corfield, the expert employed by the milk company, has published in a medical contemporary, proves that there were ample means and opportunity for contamination of the water used in the dairy operations at the farm from the privy in which the excreta from one, if not two, patients suffering from enteric fever were received; but Dr. Corfield does not say one word as to sewage irrigation or the employment of putrid manure in the pastures. On one occasion a quantity of blood and putrid matter, discharged from the bowels of the farmer, who died in June, was buried by order of the doctor in a field immediately outside the farm premises. But burying in one isolated spot is quite a different thing from manuring by sewage, and there is no evidence that the cows were fed in the field in question. On the other hand, there is a direct communication by soakage from the privy to the well, and the water of this well, which supplied the pump used in the dairy, is so bad that it has long been condemed by

medical men as unfit for drinking purposes, and was not so used by the residents on the farm. Besides the soakage from the privy, the well is so situated as to drain the whole premises, including soakage from a ditch which received fluids from the pigstyes, the dairy, and a manure heap on which the house slops were thrown. Anything worse than the arrangements, from a sanitary point of view, could scarcely be conceived. The epidemic itself is an instance of widespread typhoid poisoning, in which the poison was collected in a well in Oxfordshire, and thence carefully conveyed and distributed in London. The middle ages believed that epidemics were produced by poisoning wells; it has been reserved for the nineteenth century to prove the fact.

We are justified, therefore, in saying that there is nothing in the facts alleged by Mr. Smee which would support the hypothesis that milk produced on land manured by sewage is capable of giving specific enteric fever, so neither does the Marylebone outbreak lend a shadow of confirmation to that hypothesis. But, separating the hypothesis from this special outbreak, it is of vital importance that the inquiry be made whether there is any evidence that sewage used as manure is capable of so affecting the produce of the land—either animal or vegetable—as to render it unfit for human food, and the means either of propagating the germs of specific disease derived from the sewage or of producing other toxic effects. *In limine*, it must be acknowledged that disease has from time to time appeared to arise in the neighbourhood of land irregated with sewage as a consequence of the pollution of the air or water. In the Medical Times and Gazette of April 17th, 1869, is a notice of an outbreak of dysentery in the Cumberland and Westmoreland Lunatic Asylum, which seems to have been due to emanations from some sewage-irrigated land in close proximity to the Asylum. In the number for May 24th of the present year, we noticed an outbreak of enteric fever at Ecton, near Northampton, which was investigated by Dr. Buchanan, and which appeared to have originated amongst some labourers who were at work on a farm irrigated by the sewage of Northampton, and who drank some water from a brook which received the water that had drained through the soil of the farm, as well as any of the sewage that had been discharged from the farm without passing through the soil. These occurrences, and others of a similar bearing, may be held to prove that sewage itself can produce toxic effects, and that sewage irrigation, unless proper care be taken, not only in its application, filtering, and deodorisation, but also in apportioning precisely the quantity used to the area over which it is spread, is not a proceeding entirely devoid of danger. But it

does not help us to an answer whether the toxic qualities of sewage are capable of reappearing and producing their effects in animal and vegetable produce.

With regard to vegetable produce, we have, on the one hand, Mr. Smee's assertion that putrid matters can affect vegetables, but whether by communicating to them toxic qualities he does not say. On the other hand, there is an enormous mass of evidence that the vegetable produce of much sewage-irrigated land is eaten both by man and animal without any ill effects accruing. In the *Times* of Tuesday, for instance, is a letter from Dr. Williams, of the Sussex Lunatic Asylum, at Hayward's Heath, who states that thirty cows and twelve horses are fed with the ryegrass grown on land irrigated with the sewage of the Asylum, and that the inmates of the Asylum get their dairy produce from the cows, and that no bad effects have ever resulted. But, besides ryegrass, beetroot, oats, and other vegetables, have been raised on sewage farms, and made the food of man and animals with impunity. We have no evidence that the poison of enteric fever can produce that fever either in carnivorous or herbiverous animals, and all proof is wanting that any poisonous effect other than enteric fever has been produced in animals by sewage vegetable produce. With regard to man, evidence is also lacking. Of course, if sewage has been used in a proportion too great for the soil to absorb and decompose, it may remain on the surface and adhere to the vegetables grown. It is conceivable that a danger might arise were such vegetable matter made human food. But we believe there is no evidence of mischief having arisen from such contamination.

The other question of possible contamination through animal produce, and especially through milk, is that of greatest interest at the present juncture. Positive evidence of the toxic contamination of milk and other animal produce by sewage we believe to be entirely wanting. It was supposed that sewage pastures would spread far and wide entozoa amongst our population, But, although theoretically this would appear probable, we know of no evidence that diseases of this kind have increased since cattle have been fed on sewage grass. With regard to dairy produce, one outbreak of typhoid in the neighbourhood of Croydon was attributed to milk from a sewage farm, but tadpoles were found in the milk, which settled the question as to the real source of contamination. On the other hand, the observations of Dr. Alfred Carpenter, of Croydon, Professor Corfield, and a number of other scientific witnesses, are unanimous as to the excellent character both of milk and butter produced from sewage grass, and we may confidently assert that the greatly preponderating weight of

evidence is against the occurrence of any contamination. But it may be dangerous to dogmatise as to its possibility. No secretion of the body is more delicately sensitive to diet than the mammary. To say nothing of the epidemics of obscure disease which in the United States are referred to milk, it is well known that milk may be rendered poisonous by cows or goats being allowed to feed upon poisonous umbelliferous and other toxic plants. It is also certain that some organic poisons may be passed through the systems of several animals without losing toxic power. Thus the blood of an animal killed with the poison of the cobra is itself poisonous, and will rapidly kill if it be injected into the veins of another. But beyond such scattered facts, which may be supposed to be analogous, there is no evidence that the germs of a specific disease, such as typhoid, can be taken into the cow's system through the channel of sewage grass, be thence excreted by the mammary glands, and, producing no toxic effect on the cow, can spread enteric fever amongst the children who drink her milk. Such a sequence of events is most likely impossible; but, if possible, there has been no outbreak of fever or other disease in this country which would warrant us in believing that it has taken place.—*Med. Times and Gazette, Aug.* 23, 1873, *p.* 204.

4.—ON A PURE WATER SUPPLY.

By the EDITOR OF THE MEDICAL PRESS AND CIRCULAR.

[There can be no question as to the convenience of using soft water, but there are grave doubts as to the suitability of soft water for the supply of large towns.]

On reference to a report of Dr. Letheby's, we find he gives the mean mortality per 1000 in an average of five years of twenty English towns, and with a water-supply of ten degrees of hardness, and in another group of thirteen with a water supply under ten degrees. In the first division we find placed nine large towns—Portsmouth, Wolverhampton, Nottingham, Hull, Southampton, Reading, Ely, Chelmsford, and Gloucester, and each, it appears, have an average death-rate of 21·9 per 1000. In the second division are the names of the towns we will not specify, but the average death-rate reaches the unenviable position of over 28· per 1000, so that here we once more meet with an undeniable proof of the superior salubrity of those towns supplied with hard water as compared with those obliged to put up with a soft-water supply. And, furthermore, as to the health of the inhabitants of such towns, we find it stated, upon good authority, that in the towns last referred to, diarrhœa prevails as a chronic complaint. In

other countries also this is the case; take, for instance, the Netherlands, or the City of Boston, United States. The latter town derives its water-supply from Lake Cochituate, and the health of the inhabitants is in a most unsatisfactory condition at the present moment. Dr. Hayes, in a paper published in Selliman's American Journal of Science tells us that the water of this languid lake contains "numerous animacules, fresh-water sponges, crustaceans, and an abundance of ochry-matter." "It is only consistent to find," says the same authority, "an affinity to putrid water, and there are periods of the year during which it closely approaches to this character." In October last "the water-supply had become most offensive, and the water-fleas had attained to an unusual size and quantity, so much so that the cotton filters were closed by their dead bodies." After such a statement as this, it will surprise no one to find by the fourth "Annual Report of the State Board of Massachusetts" that Boston is most unsanitary, and that typhoid fever, diarrhœa, and dysentery, are sweeping off thousands; that there is filthiness within and squalor and disease without; and that the waters from the foul streams are mingled with human excrement, nevertheless they are habitually used for drinking, and washing the clothes of the people who live in more aristocratic quarters.

Few who have followed the water inquiry with any attention will fail to see that collateral evidence and argument in every way demonstrate the superiority of hard against soft water, and will understand why the Commissioners of Paris and Vienna decided in favour of hard water for the inhabitants of these cities. Of the waters selected for Paris we may point to those of St. Philbert and Armentiéres, brought from a distance of about eighty miles, and each with an average hardness of 12·46 per gallon; while for Vienna, the famed Kaiserbunnen and Stixenstein show an average hardness of 10·82 per gallon. It is scarcely credible, with such facts staring us in the face, that any man, or any body of men, should be found ready to bring surface water into our towns and pronounce it fit for every domestic purpose. Nevertheless, this was attempted by the Town Council of Edinburgh, and it was only after days of anxious and expensive work that the Bill for the St. Mary's Loch scheme was rejected. The experts, among whom we find the names of Professors Sir Robert Christison, Voelcker, Dr. Letheby, Mr. Jabez Hogg, and Mr. Hawkesley, proved to the satisfaction of the Lords that the water of this lake is largely contaminated with organic matters, and totally unfit for domestic purposes. Dr. Letheby, and the chemists with him, supplied the chemical analysis of the St. Mary's Loch waters, and with the following results. It must be first explained

that at least 7,000 acres of peat land drain into St. Mary's Loch, and there is no means adopted for separating the pure water, which arises from certain springs, from that made impure further down, and therefore the dwellers in the cottages standing near the edge of the Loch suffer very much from diarrhœa and fever. Dr. Letheby demonstrated that the water taken from St. Mary's Loch acted injuriously even upon lead, and when left in contact with it for a time it became quite milky in colour. Upon adding a few drops of sulphuretted hydrogen to a sample standing on the table before the Lords, it immediately turned brown, which at once proved that the water contained a most dangerous quantity of lead contamination. In Glasgow, at one time there was a good deal of lead colic and lead poisoning from impregnation.

Sir Robert Christison says of the St. Mary's Loch water, that there was always danger in using it, and this remark applies to all the water of the Scotch Lochs, as such waters "have a tendency to cause affections of the bowels of a virulent character, stomach fever, &c." He, in support of his view, cites the authority of a distinguished French physician, who was with the Poles in their patriotic war of 1831, and who furnished him with a number of remarkable instances of illness throughout the campaign. In his opinion, "the only purpose for which such water could be applied was for cleaning the streets."

Mr. Hogg's microscopical examination of the water of this lake quite confirms the view entertained by the eminent authorities already quoted. He says, "it is almost impossible to believe that the water of St. Mary's Loch can be converted into a wholesome potable water. It certainly could only be made fit for domestic uses by costly chemical and filtering processes. The impurities in it, and which result from the large amount of organic matter held both in suspension and solution, are common to most surface waters; during the warm weather the decomposition of the various vegetable and animal matters must always be largely productive of diarrhœa, typhoid, or gastric fevers."—*Medical Press and Circular, Aug.* 27, 1873, *p.* 193.

5.—ON THE DANGERS OF WELL WATER.

The dangers of bad milk are engrossing so much attention just now that there is reason to fear lest the far greater dangers of bad water should, for the time, be overlooked. We trust this serious error will not be committed. For one sample of dangerous milk a thousand of dangerous water

could be obtained in almost any part of the country. Let it never be forgotten that very few rivers or wells are safe sources of water-supply, and that many are as unsafe as loaded fire-arms. The shallow wells of villages are among the worst pests of the country, and it is high time that a zealous and well-organised crusade should be brought to bear upon them. It is sickening in most country places to observe the uniformity with which the cesspool and well are made to stand side by side, as though each was necessary for the other; and to think of the twenty feet or so of foul sewage-reeking soil through which the water percolates to its fetid bed. The question should engage the earnest attention of every health officer, and will in too many cases tax his energies severely, for it is one of the hardest of sanitary problems. It is always possible to provide a city or town with good water, but in a village, where houses are few, money scarce, and intelligence scarcer, it is often a matter of exceeding difficulty. — *Lancet*, *Aug.* 30, 1873, p. 312.

6.—ON POISONING BY LEAD IN DRINKING WATER.

By Dr. J. W. Von Tunzelmann, Wimbledon.

[It becomes a very serious matter for the public when water passing even only through leaden pipes can become impregnated with lead. We have had a similar case in our own practice.]

Case 1.—I was requested, on June 11th this year, to see the cook, in one of the best houses in the outskirts of Wimbledon, and found that she had vomited everything that she had taken (according to report) for about three weeks. She vomited also a green bilious-looking watery fluid frequently, but mostly at night. There was a yellowish tint of the skin of the face, and the conjunctivæ were decidedly yellow. There was also constipation, but there was no pain either in the right hypochondrium or at the epigastrium. The region of the liver was not tender to pressure or even strong percussion, and the region of hepatic dulness was not increased. The urine high-coloured, but not saffron-coloured. The case seemed a puzzling one. I was told that she had had a similar attack four years previously, which was then attributed to the smell of fresh paint in the house. In a few days the vomiting was considerably relieved, and she was able to retain some cold beef-tea, but only in very small quantities at a time. This went on till July 14th, when she was so much better—the bowels also acting regularly—that I expected soon to leave her quite well, and found her still progressing on the 21st. But on the 23rd

the vomiting returned, and on the following day she was worse than ever. The weather meanwhile had set in very hot, and I could only attribute this relapse to that; but still there was no pain in the region of the liver or at the epigastrium, and none in the abdomen except when she retched much, in its lower part, evidently of a myalgic nature, from fatigue of the abdominal muscles. The vomiting continued for some days, and she became so weak that I was obliged to give her champagne, which she relished greatly, taking about half a bottle a day without vomiting it. This she continued about a week, and then began to take beef-tea in small quantities; and, subsequently, the weather becoming cooler, she improved rapidly, and up till August 6th there had been no vomiting. On the 9th also she was still free from it, but complained of a loss of power in her hands, so that she could scarcely do anything with them. All this time the patient had not been confined to bed, though obliged to lie on the bed the greater part of the day on account of excessive debility. As this form of paralytic weakness of the arms is a well-known effect of lead among painters, and as this illness had been very anomalous throughout, I examined her gums and found the "blue line" most distinct along the alveolar border of the lower jaw. I therefore took home some of the drinking water furnished by a well in the house, and, testing it with a solution of sulphuretted hydrogen, found that the water became of a distinctly brownish tint, showing the presence of lead. I also took a specimen to Dr. Frankland, of the Royal College of Chemistry, who tested it, and stated in his report to me about it that it contained "so much lead as to render it quite unfit for domestic use;" but he also told me that he had never had to analyse well water for lead before.

The house is a semi-detached one, and there is a common well midway in front of the two houses, from which a leaden pipe leads to the kitchens, a distance of about twelve yards on each side.

Case 2.—In the adjoining house, the housekeeper had in the winter (in February) anasarca of both legs, which continued for about three months. There was no albumen in the urine. The anasarca was relieved by treatment, and she continued free from it from the end of May till the beginning of July, when it returned, and was again relieved in about a fortnight; this also, I have no doubt, was caused by the lead. She used to complain frequently of excessive nausea. She has not the "blue line" on the alveolar border of the inferior maxilla, but she has a leaden hue of skin, and the buccal mucous membrane also has a leaden hue.

August 18th. I was sent for to-day in great haste to this patient because she was suddenly seized with a loss of power in her hands while occupied with household duties (she awoke last night with a numb feeling in them which frightened her). The mistress feared a paralytic stroke; but I was able to relieve the mind of the lady, by telling her that the state of the servant was owing to the lead with which her system had become saturated, and that I did not expect that it would get worse now, as the water from the pump had not been used for four days.

19th. Better, but complains of a numbness in her legs as well as in her arms.

After this she quickly improved, and now (September 18th) only complains of neuralgia in her legs.

In the adjoining house (No. 1), where I had the case of "lead jaundice," I have been since told that a young gentleman, a relative of the family, was seriously ill last summer with what was described as "most severe colic, almost amounting to inflammation of the bowels"—evidently lead colic, though not recognised as such by the medical man in attendance on him (from London).

The lady told me that at Christmas, having for some time had great difficulty in getting the pump to act, they had it examined, and found that the pipe (a leaden one) was completely "corroded away" in some places. It was replaced by a new leaden one. The owner of the houses is now going to have the leaden pipes removed, and replaced by patent pipes of lead lined with tin, on the recommendation of an architect of eminence.

Case 3.—I have at present also under my care a young lady who has had diplopia to a most alarming extent for about three months, owing apparently to a sluggish action of the muscular apparatus of the eye (it is very apparent in the movements of the iris), which at first puzzled me completely, the health otherwise being perfectly good. I now find that this case also is owing to the water of the well being strongly impregnated with lead. She suffers from excessive languor. The house is about half a mile from the other two, but on the same hill, and no doubt the water is of the same quality. When the symptom first showed itself, after having existed for about three weeks, she was sent to Hastings for a while (about three weeks), and improved while there. When she returned home the improvement ceased to progress. She then went away again (to Chiselhurst) for three weeks, and improved so much as to be able to drive a pony phaeton, which she could not do even while at Hastings, on account of the double vision. After being away for three weeks she returned, and again the

improvement ceased to progress. She is now going to the Lakes for six weeks, which will, I have no doubt, remove the remaining trouble, while the well will be made fit for use by having the leaden pipe, by which the water is drawn from it, replaced by a pipe of galvanised iron.

Case 4.—The mother of this young lady consulted me on August 15th for pain in the back (lumbar region), with very abundant pale urine. I had not then tested the water of this well, but suspected it to contain lead. After I had tested it, and found the lead, I told her of the other cases which I was attending, and she then informed me that she had for some time had an unpleasant numb feeling in her hands and arms, evidently the commencing lead palsy.

These cases remind one strongly of the celebrated Claremont poisoning cases which happened in the same county, being almost within sight of this place.

August 30th. Dr. Frankland has determined the quantity of lead contained in the water of the well which supplied the semi-detached houses in which cases 1 and 2 occurred. He finds it is 1·547 grains per imperial gallon—*i.e.*, 55 per cent. more than the water which caused the alarming symptoms among the late ex-royal family of France, at Claremont, about twenty-five years ago.

The general quality of the water is shown by an analysis, with which I have been favoured, of the water of the well of a house situated about ninety yards from the house in which cases 3 and 4 occurred, made by Dr. Medlock at the end of last year. Two specimens were examined. One was dipped directly out of the well; the other drawn through the pump; but the water had been allowed to lie all through the night in the pipes before it was drawn off. "The specimen taken direct from the well contains about twenty-five grains of mineral ingredient per gallon, consisting of chloride of sodium and sulphate of lime; there are only traces of carbonates; there are present traces of ammonia, nitrous and nitric acids. which cause it to act very strongly on lead. *Per se* the water is in every respect fit for domestic use, but the sample which had been left in the leaden pipe all night contains a dangerous quantity of lead, and ought not to be used either for drinking or cooking."

Dr. Medlock advised the substitution of block tin pipes for lead. The owner of this house was led to suspect the presence of lead in the water of his well by perceiving an unpleasant effect in his throat after drinking the water, together with having heard that in a house close by the leaden pipe of the well had become corroded. This is the fourth well to my knowledge, in a small area, where the same deleterious influ-

ence has been at work. The houses in which my cases occurred have been built about fifteen years. The soil of the hill on which they stand is a light gravel of considerable depth resting on clay (the "London Clay" of geologists); it contains, no doubt, lime in very small quantity only.

The importance of these cases consists in the fact that the character of the water of surface-wells is now known to depend entirely on the nature of the soil through which the rainfall percolates; and as there are other districts, especially in the neighbourhood of London (all the different commons and heaths for instance), where the geological formation is essentially the same, the same deleterious influence may be at work in houses supplied by wells in them, though in many cases no doubt unperceived, on account of the quantity of lead held in solution by the water not being so great as in these cases, and, consequently, not having produced such grave disturbance of the health in any of their inmates.

I should recommend that wherever obscure cases of gastric disturbance or of paralytic or dropsical symptoms occur, the drinking-water should forthwith be tested for the presence of lead.—*Med. Times and Gazette, Sept.* 27, 1873, *p.* 352.

7.—REPORT OF THE ANALYTICAL SANITARY COMMISSION ON DISINFECTANTS.

By one of the EDITORS OF THE LANCET.

The possibility of another cholera epidemic renders it our duty once more to invite our readers' attention to the subject of disinfection. In doing so we are not anxious to state new views, or to discuss the subject fully from the point of view of pure science. We merely aim at supplying practical hints for practical purposes, and therefore do not hesitate to place before our readers much that is already familiar to them.

In pursuance of this aim we have collected and examined some of the more important of the disinfecting agents now in use, and propose to point out as briefly as possible the advantages and disadvantages peculiar to each. Before doing so, however, it is necessary to advert to the present state of our knowledge in regard to the action of disinfectants generally, and to the conditions under which they are required. The state of our ignorance would, unfortunately, have been the more accurate phrase, in spite of the great amount of really good work that has been done in the last few years.

The term "disinfectant" is somewhat loosely applied to substances possessing respectively three distinct properties. These substances may be more accurately classified as follows:—

1. *Deodorisers.*—Substances which remove noxious smells without necessarily destroying the true noxious matters which accompany those smells.

2. *Disinfectants proper.* — Substances which destroy the noxious matters arising from fermentation, putrefaction, or the infection of disease, by some direct chemical process which converts them into simple and innoxious bodies. Chlorine and oxygen are typical agents of this class.

3. *Antiseptics*, or *colytics* (as Dr. Angus Smith prefers to call them).—Substances which destroy low forms of organic life, and which therefore prevent or arrest putrefaction and fermentation, and deprive of activity all disease germs. Corrosive sublimate and the tar acids (carbolic and cresylic) are examples of powerful antiseptics.

It is impossible in this place to avoid a passing reference to the zymotic theories of fermentation, putrefaction, and infection, which have been so keenly contested of late years. The brilliant train of research, originated by Pasteur, has left no doubt of the active agency of organised bodies in the two former of these processes, and although the zymotic theory of epidemic disease is less surely established, it has been supported by so considerable a collection of facts as to have acquired a certain degree of probability, to say no more. Indeed, the action of antiseptics is itself an argument of some value in its favour, for if a certain agent—carbolic acid, for instance—stops fermentation—stops putrefaction, and stops the spread of infection, it is difficult to avoid applying to the third case the reasoning known to be valid as regards the first two. For our present purpose, at any rate, it is convenient to assume the truth of the theory, even where proof is still imperfect, and this we have already done tacitly in our definition of antiseptics.

In studying the action of disinfectants proper (for which a distinct name might advantageously be coined), we soon meet with one of the greatest difficulties that beset the subject of disinfection. For disinfection to be effective, a definite and often a considerable quantity of the disinfecting agent must be used. If the supply be insufficient we may, and often do, have deodorisation, but not a true disinfection. For unfortunately, the most deadly—that is, the organised portions of the noxious matters to be dealt with—are not those which are most easily attacked. The foul-smelling gases and vapours formed during putrefaction or exhaled from animal exuviæ are destroyed before the organised matter; as an insect or a bird would be unharmed by the presence of a quantity of chlorine or nitrious acid in the atmosphere, which would instantly destroy or com-

bine with such compounds as sulphuretted hydrogen or ammonia. This is the difficulty which we have so often dwelt upon in regard to water. When sewage is discharged into a river, the organic matter is gradually oxidised at the expense of the dissolved oxygen of the water; but even when the whole of the lifeless organic matter is destroyed in this way, we lack security that the organised or living matter has likewise been destroyed, and this is just the portion that we fear. The disinfectant in this case is oxygen, and we see that its perfect action depends on two conditions—on quantity, namely, and time.

Another great difficulty in the selection and use of disinfecting agents arises from the various circumstances under which they have to be applied. The physician is called upon to disinfect air, water, houses, ships, and clothes, and in every one of these cases the conditions may vary considerably. The offensive cause may be for the time irremovable, as during the progress of an infectious fever. Very commonly the operations have to be carried on in the presence of patients and friends, who must not be poisoned. The disinfection of the floor of a house into which sewer gases find entrance is a very different thing from the disinfection of a house after scarlet fever, and the disinfection of a cesspool or foul pond is different from either. Dr. Angus Smith, with his usual penetration, has drawn attention to the great variations that are produced in the action of disinfectants by the presence of considerable quantities of water, and other similar modifying influences might be mentioned. These considerations are familiar to all who have studied sanitary science, and serve to remind us of the vast difference between mere theoretical knowledge and the practical skill which comes of common sense informed by study and trained by practice.

We now pass to an account of the chief chemical agents which can be made available for purposes of disinfection, selecting those only which have, or claim to have, practical value and importance.

1. *Chlorine.*—This agent deserves the first mention, on account of its extensive use, and great value under certain circumstances. As before said, it is a disinfectant, and it acts either by removing hydrogen from organic compounds, or by promoting oxidation by withdrawing the hydrogen from water. It is commonly used in the form of chloride of lime, a compound which readily yields chlorine when treated with strong acids, and which slowly yields hypochlorous acid, an agent still more powerful than chlorine, when exposed to the carbonic acid of the air. Nevertheless it has been distinctly proved that the value of chloride of lime, and even of free chlorine, has been

greatly over-rated. It is true that very small quantities of chlorine will deodorise, but a good deal is required for the destruction of organic life. The common practice of placing saucers with chloride of lime in bedrooms, closets, &c., is not only useless but dangerous, as tending to induce a false confidence, and even when a faint chlorous smell is produced by the addition of hydrochloric acid to chloride of lime, or peroxide of manganese, or by the immersion of crystals of chlorate of potash in hydrochloric acid (all of which processes have been recommended), the result is equally ineffective. The only true use for chlorine in disinfection is for the disinfection of houses, clothing, and bedding after disease. For this purpose the rooms must be closed up, and the chlorine generated in such quantity as to prevent the possibility of human beings entering for many hours. The following is the formula given by M. Regnault, and used in Paris after the siege for the disinfection of linen and bedding. We give English weights and measures approximating to the originals. One pound of chloride of lime sewn up loosely in a strong canvas bag is immersed in a mixture of a pint and a half of common hydrochloric acid. with four pints and a half of water. The room is then closed, and the goods exposed to the gas for twenty-four hours, after which the room is ventilated for forty-eight hours. Everything may then be considered safe. The action of chlorine on delicate metal work and coloured fabrics must of course be borne in mind.

2. *Nitrous fumes.*—The brown fumes of peroxide of nitrogen, produced by the action of nitric acid on copper in the presence of air, were found by a recent special commission of the French Academy of Sciences to be more powerful than chlorine as a destructive disinfectant. For a room of 1000 to 1500 cubic feet the following are the proportions recommended. Water, half a gallon; ordinary nitric acid, three pounds and a half; copper, three quarters of a pound; the whole in a two-gallon earthenware pan. Doors, windows, and the fireplace must be carefully closed with gummed paper, and extreme care used in entering the room, as the fumes are most deadly. The commissioners, however, go on to say that carbolic acid, which is far less expensive and noxious, and more convenient, is, when properly used, equally efficacious.

3. *Ozone.*—Although oxygen is nature's great disinfectant, its operation is too slow to render it available for the purposes of our present study. The condensed and peculiarly active form of oxygen known as ozone, has, however, long been known to be a rapid and energetic disinfectant of the destructive kind. But ozone was, until lately, only to be procured in

small quantities, and by inconvenient or expensive methods. The invention of Siemens' tube with the modifications which it has since undergone, renders it possible, at the present time, to procure considerable volumes of ozone at comparatively moderate cost. The question has therefore arisen in many minds whether this agent may not now be utilised for purposes of disinfection. Without pretending to dogmatise on the subject, we venture to think that any such application on the large scale is at present improbable. The cost of producing ozone would still be considerable; it would be useless except in very considerable quantity. In a concentrated state it is extremely disagreeable and irritating, and we are not aware that it possesses any great advantage over less expensive and more convenient agents. We should not have included it in this survey but for the prevalence of loose talk about it; talk which is calculated to foster extravagant ideas of its value.

4. *Sulphurous acid.*—Hitherto we have considered only gaseous disinfectants proper—that is, of the destructive kind. We now come to a gaseous antiseptic, very powerful in its action, cheap, and easy to apply, but requiring, like the preceding gases, great care in its use. For purposes of fumigation we are inclined to think it superior to all other agents, except perhaps the tar acids, with which, however, it may be associated. In the excellent researches of Mr. Crookes on the use of disinfectants in preventing the spread of the cattle plague (Chemical News, 1866, vol. xiii.), the greatest advantage was obtained by the conjoint use of sulphurous and carbolic acids (the former obtained by the burning of sulphur), and it was a point of special interest in the experiments that marked advantage was obtained by the use of the gas in quantities too small to be prejudicial to men or animals. It would appear from this that an antiseptic may do good in small quantities, when a disinfectant, under the same conditions, would be worthless—a result confirmed by many observations on the action of the tar acids.

For sulphur fumigation, when human beings and other animals are excluded, we are advised to burn 4 oz. of sulphur to every 100 cubic feet of space. This quantity is probably unnecessarily large, and it would be rather difficult to keep the sulphur from going out. It might, of course, be heated over a chauffer, or made up into torches, which last we should think a plan well worth trying.

Some of the sulphites, notably those of soda and lime, are used as antiseptics; but for purposes of disinfection they do not appear to be very valuable.—*Lancet, July* 26, 1873, *p.* 119.

8.—THE PERIOD OF INFECTION IN EPIDEMIC DISEASE.

From the "PROCEEDINGS OF THE EPIDEMIOLOGICAL SOCIETY, 1872-3."

An important and suggestive paper, on the period of infection in epidemic disease, read before the Epidemiological Society by Dr. William Squire, throws considerable light upon one of the most obscure questions in medicine. It is based chiefly upon personal observation, and opens out a field of research which deserves to be much more fully cultivated than it has yet been. Commencing with the proposition that in contagious maladies the infection begins as soon as the disease begins, and lasts for a variable time after it is over, the inquiry proceeds to elucidate the intricacies of the initiatory phenomena of the more ordinary epidemic disorders, with the view of guarding against infection in the earlier stages of some of them, and to bring together facts illustrative of the persistence of infection in others. The paper thus falls naturally into two parts—the first occupied with general views on the incubation of disease; the second with details of the time occupied in the development and propagation of many of the more ordinary forms. This latter part will not take up so much of our space as the former, for its value consists in embodying exact data of the intervals of infection in measles for upwards of forty cases, with similar facts for a good number of cases of mumps and chicken-pox, besides some suggestive examples in hooping-cough, scarlet fever, and typhoid, sufficient to show how much has still to be done for these diseases. In measles it is not too much to say that the record here given by Dr. Squire is as valuable for comparison and reference as that given for typhus by Dr. Murchison in the St. Thomas's Hospital Reports for January of this year.

A reason for entering at length into the incubation period of epidemic disease is that some diseases, such as measles and hooping-cough, are propagated almost as certainly while in this preliminary stage as when fully developed, and that both these complaints are often insidiously introduced into homes where it would be specially desirable to prevent their entrance, by want of attention to this fact; so that the author is at some pains to enforce the axiom that "diseases may be infectious before they are developed," and that the key to this apparent paradox is in the study of the incubation period. The two separate stages of latency and of invasion, into which the incubation period is divided, give the one the time necessary for the ordinary precautions of quarantine, the other the time at which infection begins. The latter is the so-called "sickening" for the approaching malady, characterised by

fever, sometimes by a predominance of the general symptoms over the local; so that though the nature of the special disease may at this time be doubtful, the degree and duration of the fever, as ascertained by the thermometer, aid towards an earlier certainty than could be afforded by the local symptoms alone.

The rapidity with which the poison of disease can become operative varies both as to the form of poison and as to the mode by which it finds ingress: thus, small-pox by inoculation requires nine days to produce its effects; by inhalation, fourteen. The whole of this time cannot be assigned to the period of latency, is in the last three or four days of the interval some of the initial stages of the disease are being accomplished. The same thing is observable in measles, which, developed in seven days after inoculation, requires ten or twelve days to appear after infection. The latent period varies with the mode by which the poison is introduced. There is no such variation after the diseased action has begun, for the phenomena of the last five days, however the poison may have been received, are identical, and of the last four sufficiently obvious to be popularly recognised. These days are shown to be infectious, as far as measles is concerned, by conclusive evidence.

It may be affirmed therefore of some diseases, that they cannot show themselves except after a certain interval, the stages of invasion occupying a definite time; that if to this there be added, as often there must be, an uncertain interval of latency, then consequently the "incubation period" of these diseases must be a long one. On the other hand, where the different processes that constitute the disease are gone through with extreme rapidity, such diseases can be classed together as having a short period of incubation. Small-pox, vaccinia, measles, rubeola, mumps, varicella, and typhoid fever represent diseases having a long incubation period; typhus and relapsing fever forming the connecting link with the class having a short incubation period, which includes relapsing fever, scarlet fever, diphtheria, plague, cholera, diarrhœa, influenza, erysipelas, and, lastly, hooping-cough, the connecting link of this group with the former, for it combines some of the leading features of both classes.

This classification of epidemic diseases is adopted with advantage in treating of their periods of infection. A long incubation period is followed by a definite illness, terminating in a crisis, with sequelæ neither very constant nor of long duration; a short incubation period gives rise to a sudden morbid disturbance, that may have either a long or a short process, very liable to relapses, and to prolonged and definite sequelæ, with infection persisting far on into convalescence. It follows

that infection is spread most at the end of the diseases of the latter class, generally from impatience of their lingering effects; in the former class, as already stated, the earliest period is often a source of danger from disregard of the premonitory symptoms. Severe as is the morbid process often met with in this group, and great as the amount of infection must be while at its height (for the amount of infecting poison must always be directly as the amount of the disease present,) yet when the diseased action is over and the past evidence of disease cleared away, as after the falling away of the crusts in small-pox, infection would seem to cease more quickly and completely than in many less terrible maladies. So also in typhoid fever, where the morbid process is more definite and prolonged than in typhus, infection ceases soon after, if not as soon as, convalescence is established; while typhus is most virulently contagious after more than a week of very complete convalescence. Hooping-cough is associated with the first group more because of the frequency with which it is communicated in its early stages, than from any valid claim it may have to be considered long in its incubation period. The stage of invasion is often considerably prolonged, but it matters little whether we consider this as really part of the disease without the hoop so long as we recognise it as an infectious part of it. The remarkable association of hooping-cough with measles gives it a claim to some notice here; statistics on a large scale show that it is so associated, and partial observation very generally remarks the connection. There is also a close parallelism in their natural history, so that sometimes a doubt may remain whether in a particular instance the two diseases contracted at the same time have been running their course together, the hooping-cough at first marked by the more prominent phenomena of the eruptive disease; or whether some of the pulmonary complications of measles would account for certain persistent symptoms common to both. Of these two explanations, the former is supported by several cases of mixed measles and hooping-cough, as well as by the analogy found in one or two cases where hooping-cough was complicated with varicella and vaccinia; while the latter would imply that the one disease might be only a phase or further development of the other. This would appear to be less likely than that after one disease has run its course there should remain a special liability or susceptibility to suffer from the other. Regarded by the light of the instances adduced, measles and hooping-cough may be taken as the two members of totally distinct groups or circles that approximate the nearest to each other, but are individually as separate and distinguishable as are measles and mumps.

When the infection of both measles and mumps is received at

the same time, the measles makes its appearance first, and the mumps shows itself about ten days after the measles has subsided. In this conjuncture, as in that of measles and hooping-cough, the ordinary incubation period of measles is prolonged; and that of varicella, judging from one of the cases, would seem to be prolonged also. Hooping-cough taken with measles is seldom fully declared in less than three weeks after the eruptive disorder, the one disease interrupting the other; the more nearly allied diseases, measles and mumps, can run part of their course together, for, allowing for some mutual retardation, the time at which the mumps follows the measles is very nearly the same as is often occupied in the development of mumps when no eruption of measles has intervened.

The period of incubation in twenty-five cases of mumps was from 14 to 18 days in fifteen of the cases; of the remaining ten cases, six occupied from 18 to 20 days, three 21 days, and one 22 days; so that for a preventive quarantine, when the poison of mumps has to be guarded against, fully three weeks must be allowed. It is noticeable in these cases, as in those of measles, that a short limited exposure to infection does not serve to fix the shortest period of incubation, but has been followed by a longer interval of freedom from all signs of disease than when the exposure has been more continuous. When measles and mumps have been received at the same time, measles, having the shorter incubation, shows itself first. To those susceptible of taking both infections, who are in contact only until the eruptive stage of the first is over, measles only is communicated. It is possible when measles and hooping-cough are combined, for measles only to be communicated to others susceptible of both. When a child insusceptible of either, from having previously gone through one complaint, takes the other, that alone is communicated to children who have previously had neither; thus, by a process of clinical analysis, these different diseases, in whatever degree they may be related to each other, are shown to be essentially distinct. The true alliance of the epidemic and contagious roseola or rubeola is with measles, from which it is sufficiently distinct, and certainly is not merely a spurious or second attack of true measles morbilis.

The concurrence of scarlet fever with measles has been very rarely observed. Even where they have prevailed simultaneously in the same establishment, the susceptible have taken the one or the other with very little modification of type. In one case a girl was thought to have slight symptoms of scarlet fever, and was removed from a school where a servant had measles. This girl soon lost her sore throat, but fell ill with measles in the following week. Some of the girls who

remained at the school were ill with saarlet fever at the same time. They of course were isolated till they could be safely removed, and did not take measles, one of them having measles in a very characteristic manner a year afterwards.

The incubation period of scarlet fever is a short one, so that a week or eight days may be all that is requisite for quarantine purposes; and early separation often suffices to prevent its spread; but the length of time infection may linger is shown to be a much longer one than is generally supposed. Two cases are given where personal infection persisted for sixty-six and seventy days respectively. It would seem, however, that infection may be received, and either lie dormant for a limited time, till some shock starts it into activity, or that its effects are intercepted at an early stage, as in a case extracted from a clinical lecture of Sir James Paget's, where scarlet fever, just appearing after lithotomy in a boy, was delayed for nearly a month, till the wound had healed; and three weeks after scarlet fever hooping-cough made its appearance. A similar prolongation of the incubation of typhoid is given, where some premonitory diarrhœa, already existing a week, ceased while the patient was under an operation for fistula. Three weeks afterwards, as soon as the wound had healed, a rigor occurred, followed by high fever and diarrhœa, characteristic of typhoid, which then ran its usual course.

The old maxim of "two diseases not co-existing" still retains much of its force. In the concurrence of two diseases we see an inhibiting power on the part of the predominant malady; the diseased processes already established interrupting or retarding others that are imminent, thus vitiating to a certain extent our calculations as to the period or duration of infection.

Another possible source of infection, sometimes overlooked, is in the second or modified attacks of many of these diseases. Modified small-pox, in its mildest form, is known to be capable of conveying the disease to others who may be susceptible of its worst effects. Persons who are said to have had scarlet fever and hooping-cough at some previous time are not safe from the liability of giving the disease to others by means of personal infection from slight sore throat or merely tickling cough, as well as by conveyance in their clothes or by neglect of personal cleanliness. The length of time that infection may cling to chambers, surfaces, and clothes, is well known.

One deduction of the greatest practical importance is drawn from observing how constantly even the most diffusible infections, such as scarlet fever and hooping-cough, spread to those nearest together, whether occupying the same bed, the same class, or the same room, if that be a small one; and how

greatly the fatality is augmented where there is either overcrowding, or where after a death from one of those diseases delay occurs in effecting the requisite separation. Our deduction is this: that even though the spread of these diseases cannot be arrested by the most careful watchfulness, the injury they inflict upon the community or the individual might be reduced to a degree that is almost inconceivable, if, to the ordinary improvements in drainage and water supply of the district, we could have sufficient sleeping accommodation and breathing space in the habitations of the family.—*Practitioner, June* 1873, *p.* 406.

9.—ON THE TREATMENT OF ENTERIC FEVER BY THE USE OF INTERNAL DISINFECTION.

By Dr. Stephen Skinner.

[In this paper Dr. Skinner draws our attention to what he considers the real value of sulphocarbolate of sodium as a mode of treatment for enteric fever. He says,]

In the Practitioner for June 1872, I read Dr. Arthur Sansom's able article on "The Antiseptic Treatment of Smallpox," and I was then so much impressed with his views on the subject of internal disinfection that I resolved on giving it a fair trial in every case of smallpox or enteric fever I could meet with. Since then I have been able to treat twenty cases of the latter disease on this principle, and the result has been so satisfactory that I consider it justifies my confidence in the method. A word as to my mode of administering the drug. I generally commence with twenty grains every fourth hour, as a dose for an adult, and then I gradually increase during the next few days to the full dose of thirty grains. Beyond this I have found that I cannot go without producing cerebral symptoms as the result, which have, however, quickly disappeared upon the diminution of the dose of the drug.

Children, I have found, require much smaller doses in proportion than adults. Where the diarrhœa has been excessive, I have combined small doses of Batty's liquor opii sedativus with each dose of the remedy, until the bowels have been quieted.

With the exception of one case, which terminated fatally on the fourteenth day of fever, all the remainder progressed favourably towards recovery.

The length of time the fever lasted in each case appears to me to bear a direct ratio to the promptitude with which the drug was administered. In those cases in which I was able to exhibit

the remedy during the period of incubation, the fever disappeared quicker than when it was given on the first or subsequent days of the disease. I have also administered the sulpho-carbolate of sodium to people with premonitory symptoms of typhoid, occurring in four instances in houses where I was already in attendance, and I have been glad to find that the disease has never developed itself; it has, as it were, aborted; after from six to ten days of general discomfort, the bowels have assumed their natural tone and the excrement its normal appearance, the tongue has cleaned, and the patients have declared themselves to be in their usual health again. In these cases I fully believe that if the drug had not been administered, typhoid fever would have been developed.

This is, however, a matter of conjecture only, and beyond proof, and therefore I hope that every one who has opportunities of observing the action of this remedy on the class of cases alluded to, will give the profession the benefit of their experience.—*Practitioner, Sept.* 1873, *p.* 176.

10.—OBSERVATIONS ON FEVER.

By Dr. THOMAS WRIGLEY GRIMSHAW, Senior Physician to the Fever Hospital, Cork Street, Dublin.

[In the following paper the leading symptoms of the several forms of fever are compared. It is an abstract of a clinical lecture delivered in Steevens' Hospital.]

The essential cause of typhus is overcrowding. Many examples of this are to be found in the various works on fever, and my recent investigations as to the distribution of fever in Dublin have led me to give this statement my fullest concurrence.

I have adduced several most remarkable and well-established cases of typhus originating from this cause. It may also arise directly from contagion, and it is by far the most contagious of all the continued fevers. A proof of this is furnished by the fact that nurses employed in fever hospitals rarely escape unscathed. Thus we find that the majority of those in Cork Street Hospital are attacked with typhus fever shortly after entering on their duties there, while cases of enteric are scarcely ever met with under like conditions.

The exciting cause of enteric fever is now well known to be connected with the introduction of decomposing fæcal matter into the system by the mouth. What may be the exact product of decomposition which gives rise to this formidable disease has not yet been ascertained. It has, however, been

satisfactorily proved that this principle, be it what it may, can enter the system in the form of contaminated food or drink, or as a miasma in the air itself. In the lecture before alluded to I have drawn attention to the fact that enteric fever has been propagated by means of water tainted by sewage, by milk adulterated with animal matter, and, as a matter of course, by fæcal matter. The recent illness of H.R.H. the Prince of Wales has been justly attributed to fæcal miasm. Many examples of the connection of this disease with the drinking of water, impregnated with sewage matter, are to be found in the reports of the Medical Officers of the Privy Council for England.

I may here adduce, as an example, an instance which I recently met with when in attendance on a large educational establishment in this city. I need not mention the institution by name, but it will doubtless be recognised by some. In one of the buildings connected with this establishment lectures are delivered, which are attended by pupils of both sexes for several hours each day. An epidemic of enteric fever broke out among the males, but none of the females were attacked. It was manifest, therefore, that the cause of the fever affected the males exclusively. These were divided into two classes; one set occupied the building in which the lectures were delivered, and the other lived in a separate house in an adjoining street. Both sets, however, caught fever, so it was manifest that the exciting cause was common to both. Now, the only condition common to both was the frequenting of the building where the lecture rooms were, but the females were constantly in the same building and did not get fever. Therefore, it was not merely the building itself, but some particular part of it, where the cause existed. This was soon discovered, for I found that the latrines which were used by the males, but not of course by the females, were so badly drained that twice daily, at the rise of the tide, the drainage was driven back into the area in which the latrines were situated, thus poisoning the atmosphere of the place, and the result was, as might be expected, enteric fever, and that too of a very fatal type. Enteric fever may spread by contagion; this, however, seems to be of a peculiar character, viz., to be derivable only from the discharges from the bowels of an infected person, which can of course be obviated by using ordinary precautions. The physician, however, in treating cases of this disease, may fearlessly approach his patient's bedside, as cases are but rare where the fever is caught in this manner; indeed, I only know of one instance where I believe enteric fever to have spread from one patient to another; that case I am sure some of you recollect.

Having disposed of the causes of typhus and enteric, we will now consider shortly the origin of relapsing fever. We naturally ask, "what gives rise to it?" The answer is, beyond all question, *starvation*. It is scarcely ever heard of except in times of want and scarcity of food. In Europe it has always followed periods of famine and dearth. In our own country its ravages aggravated the horrors of the famine years, 1817, 1846, and 1847. Later than this, it appeared in Silesia under similar circumstances, and I believe that Lancashire was only saved (and that not entirely) from a similar visitation, as a sequence of the cotton famine of 1862 and the following years, by the munificence and liberality of the British nation during that great crisis.

With all due respect to the great physicians who were my predecessors in the treatment of fever in this country, I must candidly express my belief that they did not sufficiently distinguish between typhus and relapsing fever.

We hear of the famine fever relapsing, and we hear of it "relapsing into typhus." Now, I cannot give my concurrence to this statement. That patients labouring under relapsing fever were attacked with typhus I certainly do believe, but I am convinced that they caught it, and too often died of it, in hospitals, whither they had been brought to be cured of their first disease.

It is no argument to say that the relapse was typhus with maculæ, although the first exacerbation had been fever without maculæ, when the patient had passed through his first fever alongside a typhus patient, from whom he had every opportunity of contracting that disease; and I must also assert, in opposition to certain authorities, who maintain that one form of fever is a certain prophylactic against another, that I have seen a patient suffering from enteric fever get typhus in precisely the same way.

The true relation between typhus fever and relapsing is this: that both are liable to spread at the same time, since dearth, giving rise to starvation, promotes one form, while overcrowding, another result of the same lamentable state of things, produces the other, and accordingly both prevail at the same time and are liable to attack even the same individual. This is also true of typhus and enteric, for where we find the causes of both in existence at the same time, we may, as in the former case, find both diseases attacking the same person contemporaneously. In proof of this statement, I may mention that I recently published, in the Irish Hospital Gazette, cases of a family, of whom some members had typhus, some enteric, and some both forms of fever contemporaneously, and when I went to examine the place from whence the patients came, I

found in the overcrowded condition of the house and the bad drainage, not to go into further particulars, quite sufficient evidence to account for the fact. I have now endeavoured to point out the main features in the etiology of fever, and our next lecture will be devoted to the examination of the symptoms of the varieties of continued fever and their differential diagnosis.

Let us examine the special symptoms of each form of fever. In doing so, our attention is naturally directed first to the mode of invasion. This may be sudden, slow, or even so gradual as to be almost imperceptible. In typhus, the patient is generally unwell for some days, and then usually a distinct rigor comes on, decidedly marking the accession of the disease. Enteric fever comes on much more insidiously. On questioning the patient, you will probably learn that he has been out of sorts for a considerable period, that he has had a bad taste in his mouth, and perhaps that his bowels are too free. He will complain generally of chilliness, but will seldom have had a shivering fit sufficiently distinct to arrest his attention. In relapsing fever a specific rigor invariably ushers in the disease, and is followed rapidly by a hot and then a sweating stage, especially if the disease does not directly result from starvation.

In treating of the special diagnostic signs and symptoms of the three continued fevers we shall consider them from fixed standpoints, viz., their connection with the various systems.

The symptoms referable to the integumentary system are those connected with the eruption. In typhus fever, about the fifth or sixth day from the commencement, a dusky reddish brown mottling becomes apparent. It is generally first seen over the folds of the axilla and front of the chest, and then spreads over the sides and the abdomen. It is not usually found at first on the extremities, but when it is, you will almost invariably find it on the arms, more seldom on the legs, and comparatively rarely on the face.

This eruption is much redder in the young than in adults. It gradually becomes more defined, and distinct spots—*maculæ*—appear. These may be raised and irregular, and even disappear on pressure. There is likewise frequently observed a kind of greyish marbling, supposed by some to be the precursor of these brownish spots. After the lapse of a day or two the maculæ become thicker, do not disappear on pressure, but continue for days, leaving permanent marks, called *petechiæ*. These are simply a series of minute ecchymoses caused by the extravasation of blood from the weakened capillaries. When the fever begins to subside the mottling gradually fades away,

but the petechial markings persist for a considerable time subsequently.

Enteric fever differs from typhus in respect to its eruption. In many cases no rash whatever can be discovered, but in the majority it is present. There is invariably some difficulty about fixing the precise date of its appearance, owing to the uncertainty that usually prevails as to when the disease actually commences. We may, however, as a general rule, look for its advent from the seventh to the twelfth day of the fever.

The character of the eruption is—A few rose-coloured spots, often not very easily discoverable (especially on the dirty and flea-bitten skins of our hospital patients). They are about an eighth of an inch in diameter, slightly raised, and disappear on pressure; they vary in number from one to twenty or thirty. They are usually first found on the abdomen and chest, but especially on the sides of the abdomen. In females, a usual situation of these spots is the breast. They keep coming out in successive crops for a fortnight, or even longer, and never change until they disappear; they may even be present for a portion of a day and then vanish. You can observe these phenomena by tracing a circle with ink round one of these rose spots, and watching it from time to time. The average duration of each spot is two or three days. They seldom, if ever, appear after the thirtieth day of the illness.

It is very different to distinguish the eruption of enteric fever from that of typhus at first, but after a couple of days the typhus eruption will be found assuming a darker colour. and failing to disappear on pressure, and, furthermore, no fresh crops ever come out. The opposite is, as we have seen the case, in enteric fever. In some cases, however, the two eruptions have been found to co-exist, but in such cases other symptoms of both fevers are also present.

With regard to relapsing fever we have nothing to remark on the subject of rash, owing to the fact that there is no characteristic eruption connected with this disease.

In the continued fevers, as in many other affections, the pulse is a most important index as to the patient's condition.

In typhus, the general range of the pulse is from 100 to 120 beats per minute. If it be much under 100 (80 or 70), it becomes a very serious and often dangerous sign, and augurs badly as to the termination of the case. It presents the usual febrile characters, is soft and undulatory, especially about the sixth or seventh day of the disease. The tension is lower in typhus than in enteric fever. Two causes have been assigned

for this low tension of the pulse. First, a flaccid condition of the capillaries; and, secondly, weakness of the heart itself. We find the force of the heart becomes diminished, the first sound is but faintly heard, and in many cases is completely lost. This is our great criterion in estimating the strength or weakness of our patient, and hence the vital importance of constant and careful auscultation. In enteric fever the pulse is frequently dicrotous, but its febrile characters are not so well marked unless the disease assumes a distinctly typhoid form. Marked weakness of the heart, similar to that so common in typhus, is not frequent, but when it does occur, it usually points to a fatal result. With result to the indications afforded by the pulse in relapsing fever, I have not seen a sufficient number of cases of this disease to be able from personal observation to put you in possession of its special peculiarities.

The respiratory symptoms would next demand our attention, but as we shall have to deal with them somewhat at length when we come to treat of complications and sequelæ, we shall for the present defer their consideration.

The digestive system suffers *comparatively* little in typhus. There is, as we might expect, great dryness of the mucous surfaces and suspension of the normal digestive functions. The condition of the tongue, however, is most important to notice. At first it becomes coated with a whitish yellow fur, extending from the centre to the sides; this becomes gradually brown and then black, and the teeth loaded with sordes. When the fever begins to subside the parched tongue begins to moisten, cracks appear on its surface, the sordes disappear, and it gradually re-assumes its normal condition. In enteric fever, the symptoms referable to the digestive system are very well marked. To commence with the tongne. It is usually white at first and red at the edges and tip, but if the case assumes the typhoid form the tongue presents all the characters of a typhus tongue. Sometimes, instead of being furred, the tongue becomes red all over, commencing with a strip of red at each side. This is not, however, to be entirely relied upon as a diagnostic sign.

Vomiting frequently occurs early in the course of enteric fever, and this, taken in connection with the other symptoms, affords a useful sign for distinguishing this disease from typhus, in which vomiting is not nearly so common.

Diarrhœa is almost invariably present in enteric fever; this is connected with inflammation or ulceration of the glands of the intestine, but of this we shall speak more particularly

hereafter. In typhus the bowels are nearly always confined, but diarrhœa may be present.

The course which the temperature follows differs in each form of fever. In typhus, the range of temperature follows the "continuous" variety, that is, continues to rise steadily during the first few days to about 104·5° or 105°, then gradually falls, without any sudden variations except such as may be due to the occurrence of complications. The highest temperature is usually reached about the seventh or ninth day.

In relapsing fever, at each exacerbation, the temperature rises quickly and suddenly to its full height, from 105° to 107°, and as quickly falls, and therefore the range belongs to the "acuminated" variety.

In enteric fever the range of temperature belongs to the "interrupted" variety, that is, rises and falls frequently. It may reach the height of 104° or more, frequently during the course of the disease having extensive daily fluctuations (sometimes of many degrees), the highest daily temperature usually but not always being in the evening.

The height of temperature is of very different import in the different forms of fever. In typhus, a temperature above 104·5° is severe, above 105° dangerous, and 106° is seldom compatible with life. In enteric fever these rises are not of such serious import. In relapsing fever temperatures of 106° or 107° seem to be of little consequence so far as prognosis is concerned.

Sleeplessness, delirium, &c., we shall discuss in our next lecture in connection with the subject of complications, and I shall conclude for the present with a few words on the duration of the three forms of fever.

Typhus usually lasts about fourteen days. Defervescence takes place by crisis, which is most commonly accomplished either by sleep or profuse sweating. There is seldom a crisis to an attack of enteric fever. The duration of the disease is most uncertain. Three weeks is the average, but it frequently lasts much longer, and the patient may continue in a feverish condition—sometimes better and sometimes worse—even for months. In relapsing fever the first attack lasts seven days, and is followed in a few days by another exacerbation. There is no rule as to the number of relapses. They may vary from two to twelve. The first attack is the worst, and each succeeding one is less severe. We have now briefly considered the more important of the three varieties of continued fever.—*Med. Press and Circular, June* 18, *and July* 2, 1873, *pp.* 529, 3.

11.—A FEW WORDS ON THE MEANS NECESSARY TO BE TAKEN FOR PREVENTING THE SPREAD OF ZYMOTIC DISEASE.

By Dr. F. PAGE ATKINSON, late Surgeon St. Bartholomew's Hospital, Chatham, and Royal South London Dispensary.

We all, no doubt, believe in the truth of the saying, that "prevention is better than cure," but how to prevent disease we are often at a loss to understand. Epidemics are not unfrequently looked upon as direct visitations of Providence, and as such to be quite incapable of being warded off; but to this it may be replied, that Providence has laid down certain laws for the preservation of health, and that if we transgress these, we must expect to suffer sooner or later. Half the illness which takes place may be shown to arise from ignorance of, or a want of respect for, the laws which nature wishes us to follow, and to be, truly speaking, as preventible as scalds and burns. It may be asserted, therefore, without fear of contradiction, that by a proper and careful study of nature's laws, we may escape from several of the numerous ills that flesh is now considered heir to. Whenever infectious disease of any kind makes its appearance, we should take a careful survey of the premises where it exists, inquire as to the ventilation, drainage, and water-supply, and see that the latter is not contaminated by infiltrations from water-closets, dust-bins, manure-heaps, &c. We should then take into consideration the number of persons occupying each room, and the means taken for separating the healthy from the sick. If there is insufficient accommodation for the sick, they should be removed to a hospital in one of the public conveyances which are kept especially for the purpose. Where death occurs, the corpse should be removed to a public mortuary, and the house and all things in it, in either of the above-mentioned cases, should undergo a thorough cleansing and disinfecting. The Sanitary Act of 1866 provides very wisely, among other things, for the prosecution of those who—1st, Use public conveyances while suffering under contagious or infectious disorders; 2nd, Knowingly convey such people in any public conveyance; 3rd, Wilfully expose themselves in any street, &c.; 4th, Knowingly let a house or part in which any person has been suffering from any contagious or infectious disorder, without having had it and all the articles in it previously disinfected.

In order to prevent the spread of zymotic disease from house to house, it is necessary in all cases to provide for—1st, Complete separation of the sick and those in attendance from those who are in health; 2nd, Disinfection of the sick-room and all articles in it, both during its occupation and after the

removal of the patient; 3rd. Disinfection of the secretions; 4th, Thorough ventilation.

In the first place, then, the patient should be moved into a separate apartment near the top of the house, with the nurse who is to be in attendance. All curtains, bed furniture, carpets, &c., should be removed prior to its occupation, and no person should be allowed to enter the room except the medical attendant. Outside the door a sheet should be hung, thoroughly soaked with a solution of carbolic acid, and the nurse should never pass this under any circumstances whatever. All articles of food should be placed under the sheet, and not taken in by the nurse till the servant has gone away. Inside the apartment, disinfection should be carried out by means of sprinkling sulphur on a live coal two or three times a day, and wafting the fumes all around, till sneezing and a feeling of suffocation are produced. By the bedside a basin should be placed for the patient to spit into, containing a solution of carbolic acid. Rags should be used for wiping the nose, &c., and should be burnt immediately afterwards. All soiled bed-linen should be placed in a solution of the acid as it is removed. Discharges from the bowels and kidneys should be disinfected by the same means; and all plates, dishes, glasses, &c., should be washed in boiling water, and thoroughly cleansed with Condy's fluid or chloralum before leaving the room. As soon as the patient is perfectly recovered, he should wash the body thoroughly over with carbolic acid soap. He should then pass out at the door quite naked, and wrap himself in a fresh blanket, which has been left on the outside. In this way he may pass into another room and dress, and then he may safely mix with the other members of the household. The nurse should act in the same way; but before leaving the room she should see that all the linen articles are placed in a solution of carbolic acid, and that the other things are hung upon lines, in order that they may undergo a thorough fumigation. She should next place an old saucepan, containing some hot coals and sulphur, in the middle of the room, and then make her exit, taking care to close the door after her. At the expiration of twenty-four or forty-eight hours, the windows should be thrown open, and disinfection may be then said to be complete.

These rules should be carried out in the case of all infectious diseases; but certain other precautions are necessary to be adopted peculiar to the particular disease we are called upon to treat. For instance, in the case of *scarlet fever*, as soon as the eruption begins to fade, or as soon as there is the slightest appearance of peeling, the patient should be thoroughly oiled all over, night and morning, for three days. He should then have a warm bath *on going to bed*, and the same process should

be repeated at the same interval of time till all signs of peeling have disappeared. When the skin is perfectly clear, he may, after taking a bath, pass, in the manner directed, into another room and dress, and then mix with the other members of the household.

The chief thing to fear in these cases is the thin skin which peels off from the various parts of the body, and floats about imperceptibly in the air. The oiling process and disinfection, if strictly attended to, entirely prevent the spread of the disease.

In order to prevent the spread of *smallpox*, it is necessary, on its very first appearance, to vaccinate all in the same house, and, if other cases occur, all in the neighbourhood. The patient should not be allowed to leave his room till all appearance of suppuration has passed away. All articles of bed furniture and clothes that have been worn, should be burnt as soon as the case is well. Keeping the apartment dark is of great use in lessening the formation of matter, and so the spread of the disease.

In *enteric fever*, the chief thing one has to do is to see that the secretions are thoroughly disinfected, for it is by this means chiefly that the disease propagates itself. Care must be also taken whenever there is the slightest tendency to a relapse.

To prevent the spread of *typhus fever*, it is absolutely essential that there should be strict isolation and thorough fumigation, as the power of infection in these cases is exceedingly great.

In *cholera*, it is the breath, vomit, and stools, that are so particularly infectious; but the disease spreads mostly by the drinking of contaminated water. The stools, therefore, should be thoroughly disinfected, and care should be taken that they are not thrown into any place where they are likely to come in contact with the drinking water. In India, the chief thing to do is to try and prevent the natives from depositing their fæces in the soil all around the villages, and those who have the disease in an incipient stage from bathing in the tanks.

Diphtheria, *erysipelas*, and *puerperal fever*, are highly infectious and contagious, and it is highly important to see that the hands are thoroughly washed in disinfecting fluid after each examination. It is also as well to avoid going from a case of erysipelas direct to examine a healthy wound. When in attendance upon a case of puerperal fever, midwifery practice should be given up for a time altogether. The nurse also in charge should be warned not to engage herself for another case until a month or two has elapsed after leaving the house. The same advice also should be given in the case of *scarlatina*.

As regards the nature of the poison or poisons which cause

the outbreak of fever, a good deal has already been written, and will still continue to be written. Some say that germs are the originators of disease; others (as Dr Elliott of Hull), that these are only the carriers of the poison, since we cannot distinguish between healthy and unhealthy pus, the skin that comes from a healthy body and one affected with scarlatina, &c. Béchamps considers the body to be made up of minute creatures called microzymes, and that when these act harmoniously, the body is in health, and the fermentative processes go on regularly; but that when these act inharmoniously, the fermentative processes are deranged, and ill health is the consequence. The microzymes, he says, are not ferments in themselves, but they produce very small creatures called bacteria, and these produce cells. The cells and the bacteria are capable of returning to the microzymic state. After death, all organic matter returns to its original elements, and the microzymes are there to carry on the work of putrefaction. According to this idea, the living animal contains within itself the essential elements of life, disease, death, and total destruction; and in order to accomplish these last-mentioned ends, it is unnecessary to suppose the presence of living germs floating in the air.

As to whether germs are or are not the cause of disease, it is still uncertain; but, nevertheless, we all admit the benefits that arise from the use of antiseptics and disinfectants, and proper sanitary supervision.—*Edinburgh Medical Journal, June* 1873, *p.* 1083.

12.—ON THE GENERATION OF TYPHUS FEVER BY OVERCROWDING.

By Dr. James Martin, Physician to the Portlaw Hospital.

About the middle of September, 1872, an epidemic of measles set in, in Portlaw, no case having occurred for several years previously.

The summer had been healthy, and no case of typhus fever had occurred for six or seven months. Early in October, the weather being damp and foggy, measles had reached nearly every house in the town, over 450 cases being registered before the 1st of November.

Every practitioner knows the prejudice the poorer classes entertain against the practice of cleanliness during an attack of measles, it being impossible to persuade them to change the clothes or wash the person of the sick. Further, amongst a working population the beds are almost always overcrowded. The consequence therefore was, that three or four children ill

of measles often lay in the same bed for ten days or a fortnight, without any ablution or change of bed clothes or garments.

The second week in October four children lay ill in one bed, all being very dirty. They were tended day and night by their mother, who took what sleep she had in the same bed. After ten days had elapsed from the setting in of the measles, the mother was seized with very malignant maculated typhus, under which she sank on the eighth day.

At the same time, in a house about 200 yards distant, with which no communication could be traced, six children lay ill also of measles. In this house the beds were tolerably clean, but fifteen persons, young and old, slept in four beds. The eldest daughter, aged eighteen years, who had measles some time before, took ill of fever, was admitted into hospital, and passed through a heavy attack of the maculated type. Each of the children, as they recovered from the measles, took the fever, were admitted to hospital, and were all maculated. The parents and grandmother, whom I remember to have had maculated typhus in past years, escaped.

At the same time also, in a clean, fairly ventilated, roomy house, fully a quarter of a mile distant from the two former, a woman, just a week after her confinement, lay in the same bed with four children in measles, trying to nurse them herself. After a week or ten days she became ill with fever, and died on the tenth day, being also maculated. No communication could be traced between this family and either of the two former. I could follow up these statements with the history of six other families where fever seized on the members, who, having had measles before, were similarly exposed to the bad effects of dirt and overcrowding, but typhus having been then established in the town, I could not be sure of the freedom from communication with infected cases. All the cases of typhus, eleven in number, recovered well, except the first, which was exposed to the dirt poison in a most concentrated form, and refused to go to hospital; and the third, who was seized with the fever within a month after parturition. A marked feature in all the cases (which, with the exception of the two fatal ones, were treated in hospital) was the severe pulmonic symptoms under which they suffered.

The facts, 1st, that so many cases of typhus occurred under precisely similar circumstances, without being able to trace any communication between them; 2nd, that there had not been any typhus in the district for a long period, and that I could not discover circumstances to link them with cases occurring elsewhere—lead me to the conclusion that this outbreak of typhus was a clear case of ochlesis in the first three

cases, and perhaps in several others. — *Dublin Journal of Medical Science, July* 1873, *p.* 36.

13.—ON THE OBSCURE ALLIANCES OF TYPHOID.

By Dr. J. P. H. Boileau, Surgeon, Army Medical Staff.

In the Lancet of the 21st November, 1868, it was stated by the editor, in his review of my article on "Fever in Malta," published in the Army Medical Reports for 1866, that to complete our knowledge of the gastric remittent fever of the Mediterranean, we only wanted pathological information. In Barbadoes a fatal case of fever was treated by me which forcibly recalled to my mind the experience I had had in Malta, and the cases of fatal typhoid recently published in the Lancet have revived in my mind the whole subject. It is very desirable that the nature of that obscure disease described by me as the "long" fever should be clearly established, and I believe the following case will help to elucidate the subject.

A young sailor of the Royal Navy was admitted into hospital at Barbadoes. A detailed account of his illness would be merely the repetition of a subject already exhausted by Dr. Marston, R.A., Dr. Chartres, 100th Regt., myself, and others who have observed and studied Mediterranean remittent, so similar was the sailor's case to that fever. I will therefore omit my notes up to the thirty-fourth day of his illness, the thirty-second of observation.

Thirty-fourth day. Progressing favourably; tongue perfectly clean and moist; bowels regularly moved twice daily; he complains only of debility; temperature has been as high as 103·2° F. (evening of ninth day); abdomen as a rule has been hot and dry. I have not once satisfied myself as to the existence of any rose spots. The boy's age is nineteen. His tongue has never had the slightest appearance of the typhoid state, nor has he had typhoid dejections. His temperature for the past three mornings has been 99·2°, and under, but—significant fact—his evening temperature has not been below 100° since the commencement of his illness.

On this day he was to have had a little chicken, so well did he appear to be, and so anxious was he to have some; but the order I countermanded, and was assured that he did not get any. I am particular in recording this matter, because if he had got the chicken we could not have done otherwise than attribute to that solid food the untimely issue of the case. On the following morning his temperature was 103·8°, and on the following evening 104·2°. In two cases of fever I have seen an

equally sudden and great elevation of temperature follow the ingestion of jelly.

Thirty-sixth day. He has relapsed a second time. He has not diarrhœa; tongue remains clean; he has not any pulmonary complication.

Thirty-seventh day. As usual, no complaint.

Forty-second day. With the exception of his temperature he appeared to be progressing favourably until 3 p.m., when he was seized with a sudden and most violent pain in the abdomen. At 5 p.m. his axillary temperature had risen to 105·2°, pulse 120, and was in great agony; abdomen extremely tender. At 7 p.m. he was considerably relieved, having taken ninety drops of Battley's sedative solution.

I need not further detail the symptoms of peritonitis following perforation. He survived for no less than six days after that fatal event. The autopsy, made six hours after death, showed the peritoneum in a state of universal inflammation, in several places adherent; the cavity containing a quantity of sero-purulent fluid; the intestines healthy, with the exception of the lower portion of the ileum, where there was attenuation and superficial ulceration. One ulcer, having the area of a threepenny piece, had penetrated through the peritoneum, and here there was a scarlet peritonitis, from which, as a centre, the general inflammation extended.

Now, the indisputable facts are these: that in a tropical island I have had one case of fever under my care which so closely resembled that known as the gastric remittent of the Mediterranean that had it been under my care in Malta it would have been described as such; that the subject of this fever died about the fiftieth day of his illness from the effects of a perforating ulcer of the lower fifth of the ileum, and that I did not see any reason to doubt my diagnosis of the case as typhoid to be correct.

From my experience in Malta, I was led to a conclusion which I expressed in my paper on "Fever" in the Army Medical Reports for 1866—viz., that quinine might be banished from the island without much harm. Since that time I have had many opportunities of observing the nature and course of typical, malarial, periodic fevers, demonstratively amenable to quinine. Remarks on those cases were published in a medical publication on July 14th, 1869. I have also had the opportunity, during a residence of more than three years in the West Indian Islands, of observing a large number of cases of fever in their climate. I may, therefore, claim to be now in a better position to judge of the nature of the fevers I observed in Malta six years ago than I was then. And I cannot but conclude that if the *gastric remittent* is not the *typhoid*

with which we are familiar in this country, it must be some variety of it; and, if this is the case, it illustrates the truth and the necessity of the warning given to us by the Professor of Military Medicine in the Army Medical School at Netley, whom I well remember cautioning us not to adopt the evil practice of calling fevers by local names. As to the fever being in any way malarial, I must still maintain the view I have hitherto held—viz., that it is not so in the slightest degree. And I now attach a greater force to the words of the editor of the Lancet than I did when first I read them. His words were these:—"After reading Dr. Boileau's contribution . . . we confess to having strong doubts as to whether some of his cases were not in reality examples of enteric fever."

It follows that in the treatment of the long gastric remittent fever of the Mediterranean we must ever bear in mind the complications which may arise in *enteric* fever. We should not deceive ourselves by the idea that we have a fever to deal with which is amenable to quinine. We should be scrupulously careful in the administration of diet; and not fail to mark the temperature of the patient at least every evening, and by its indication to "govern ourselves accordingly." I need scarcely say that I fully endorse Dr. Latham's opinion, that it is only after the evening temperature has remained on at least two successive days below 99° F. that we can be sure the ulcers have healed, and that solid food may be given without risk. I may add that I do not remember ever having seen a relapse after such a condition.

As a result of many thousands of observations on the temperature of the white man in various climes, with shade temperatures as high as 105° F. and as low as 20° F. below zero, I have been led to the conclusion that the normal axillary temperature of the healthy adult is between 98° and 99° F: in fact, it seems to me that this temperature is as valuable and as constant a characteristic of his human identity as is the structure of his bones. The fact may be already established; but as the idea that human temperature is one degree higher in the tropics than elsewhere has been widely entertained and supported by high authority, I here mention the fact that I have observed.—*Lancet, Aug.* 23, 1873, *p.* 258.

14.—ON THE DISSEMINATION OF ZYMOTIC DISEASES BY MILK.

By Dr. JOHN DOUGALL, Medical Officer of Health for the Burgh of Kinning Park, Glasgow.

That enteric fever has been disseminated over wide areas by milk supply is now an established fact, in support of which we

have Dr. M. W. Taylor's paper on an outbreak of enteric fever at Penrith; Dr. Ballard's and Dr. Robinson's reports on similar outbreaks, one at Islington and two at Leeds; also Dr. Russell's recent report on an outbreak at Parkhead. That scarlatina has been disseminated by milk was satisfactorily shown by Professor O. Bell, of St. Andrews, and quite recently by Dr. Robinson, of Leeds. Such being the case, I have attempted to examine the matter scientifically, and if I have partially succeeded in placing it in a clearer light I shall be satisfied. It may be here remarked that it seems to me highly probable that, if milk can propagate typhoid and scarlatinal contagia, it can also propagate the contagia of the other zymotica. Proceeding on these premises, I shall consider—1st, The probability of zymotic poison being generated in milk *de novo;* 2nd, The probable nature of zymotic poisons; 3rd, Is milk a congenial soil for the propagation of zymotic poison? 4th, How may milk get infected with zymotic poison?

The Probability of Zymotic Poison being Generated in Milk.—In regard to this, I may first state that many distinguished physicians believe in the spontaneous origin of specific physical contagia. Cow's milk consists chiefly of casein or curd, fat, sugar, water, magnesia and calcic phosphates, sodic chloride, potash, &c. The colostrum, or milk, secreted for a few days after parturition, differs from normal milk in containing 15 to 25 per cent. of albumen, with less of the other constituents. We have then, in normal milk, a fermentable or putrescent body—casein, and a fermentescible body—sugar. Were the casein absolutely removed from milk, the sugar would remain unchanged, because sugar in aqueous solution only and always ferments by contact with a body fermenting or putrefying. Were the sugar entirely removed from milk, the casein would still putrefy, the cause of which will be explained under the third head. But in the manufacture of cheese, which consists largely of casein, its putrefaction is prevented by the abstraction of water, the exclusion of air, and the rendering of the mass homogeneous by pressure. Milk then, from its proximate chemical composition, must be, and as we know is, eminently prone to decompose. Let us examine briefly the rationale of this decomposition. Milk newly drawn from any herbivorous animal is faintly acid, or neutral, or slightly alkaline in reaction. In the carnivora, the milk is said to be always slightly acid. When allowed to stand for a few hours, the fatty matter or cream rises to the surface, which, under the microscope, is seen to consist of highly refracting fat globules, known to have albuminous envelopes If the weather be warm, it may be found at the same time that the milk has become acid and coagulated, the sugar converted into lactic acid (by that por-

tion of the casein nearest the surface having become putrid, and acted on the sugar as a ferment), and the casein, being insoluble in an acid solution, ultimately separates in clots and flocci. We have now a markedly acid organic solution to deal with; and as I have clearly proven, in my recent paper on "Putrefiers and Antiseptics," in the Glasgow Medical Journal (Nov. 1872, and Feb. 1873), all acid organic solutions (if not hyperacid) constitute the most congenial soil for the propagation of fungi. Now, as many hold that zymotic diseases are propagated by cryptogamous spores, it is worth considering whether it is probable that the fungi which appear (generally in about two days) in fermenting milk may at any time be the cause of these diseases.

The Probable Nature of Zymotic Poisons.—These, of course, must either be vegetable, animal, or mineral in their nature, and appear either in a solid, liquid, or gaseous condition. For many obvious reasons it is impossible they can be mineral or inorganic; hence, they must either be vegetable or animal; and, assuming that there are no gaseous nor liquid animals nor vegetables, it follows that they must exist *per se* as solids. Whether vegetable or animal solids, however, it does not follow that they must necessarily be either fungi or microzoa, or both; nor because their normal conditions are neither gaseous nor liquid, that they are incapable of suspension in air or in fluids. All analogy in the vegetable and animal kingdoms points to fungi and microzoa as the effects, rather than the cause, of the conditions of their *habitat*, and indicates that their functions may be benign rather than malignant. Quite recently Drs. Cunningham and Lewis, in India, have carefully examined the blood of persons suffering from cholera, and failed to find in it either fungi or microzymes, actual or potential; and Dr. Beale, a most distinguished microscopist, and who has devoted great attention to this subject, says that "when bacteria and fungi do appear, they are but harmless concomitants of the disease, and that they cannot be regarded as its cause any more than the vultures which devour the carcase of a dead man can be looked upon as the cause of his death." Furthermore, "that the active contagious virus does not exhibit the properties of any vegetable or animal parasitical organism yet discovered: neither can any organism, having specific and peculiar characters, be developed from any specific virus." Hallier, on the other hand, contends that fungi and microzymes are not only the cause of putrefaction, but also of the various kinds of infection, each kind having its own specific microzyme. Micrococci are found by him in every infectious material—in the alvine discharges of diarrhœa, cholera, dysentery, and enteric fever—sometimes slightly, sometimes in vast

numbers. In measles, they are found in the sputa, in small-pox, and cow-pox, in the pustules, in certain fevers in the blood, and there is no disease in which they are so abundant as scarlet fever. As an example of Hallier's views, he states that cholera in India is caused by swallowing a fungoid organism, urocystis, which constitutes the smut of rice. He found that by steeping rice in cholera stools, and after sowing it on suitable soil, urocystis was developed; and he concluded that, therefore, the fungus was the cause of cholera. These experiments were suggested by a theory of the cause of cholera, which may be stated as follows:—Cholera originates on the banks of the Ganges, where the rice plant abounds and flourishes. The contagium particles of cholera are manufactured in the cysts of a urocystis, which is parasitic on the rice plant, by the conversion of the protoplasm they contain into microzymes, so that the same material which, when brought into contact with the germinating rice grain, produces urocystis, generates cholera when introduced into the intestines. Dr. Lewis, however, easily and effectively overturned Hallier's theory, by showing urocystis could be successfully cultivated on the dejecta of persons in perfect health. It may further be stated, in confirmation of the view that microzymes do not constitute specific zymotic poisons, that in all cases where putrefaction is at its maximum stage in infecting matter, the property of infection in the given substance is lost, and in this respect the functions of bacteria are certainly benign.

Zymotic units must be excessively minute, whether composed of living solid particles, or of dead albuminoid putrefacts. Though not gaseous they are capable of being suspended unseen in air, and, though not fluid, they are susceptible of being mixed invisibly in water. According to Dr. Burdon Sanderson, bacteria germs must be less than the 100,000th part of an inch, because in water known to be zymotic they were not seen with the highest powers of the microscope, which is just saying that they only exist inferentially. If particulate, it is certain that they must be smaller than a blood corpuscle—that is, less than the 1-4000th of an inch in diameter—because, if larger, or of equal size, they could not penetrate the membrane of the pulmonary cells when inhaled, nor the vascular tunics when swallowed, to pass into the circulation; as what keeps the blood corpuscles *in* must keep particles of larger or of equal size *out*. If, however, zymotic contagia consist, as some think, of minute entities of putrid amorphous protoplasm, or, as I should call it, *pathoplasm*, then they need not be so small as a blood corpuscle, because mere contact with the pulmonary membrane, or any moist absorbing surface of a susceptible organism, would be sufficient to initiate, according to the

physical theory of Laplace and Bertholet, and to the physico-chemical theory of Liebig believed in by Bastian, Beale, and ably differentiated and correlated in all its pathological ramifications by Richardson, a specific blood metamorphosis conform to the nature of the primary contagium, which may be that of small-pox, cholera, scarlatina, measles, typhus, typhoid, &c. Still the simple question, What is a zymotic poison? it must be confessed, has still to be answered. The latest solid contribution to our knowledge of the subject has been made by Chauveau, and confirmed by Sanderson. These physiologists have clearly proven that the only part of vaccine and variolous matter which is contagious is the solid particles, which are not larger than 1-20,000th of an inch in diameter, and not the liquid. By an ingenious application of the law of diffusion of fluids, they managed to separate the solid from the fluid parts, and it was found that a child may be vaccinated or inoculated successively with the fluid parts, but without effect, while the most minute portion of the solid parts that can be separated is amply sufficient to originate all the phenomena of the cow-pock or of variola. But, supposing zymotic contagia to consist of minute particles, as in the case of vaccine and variolous lymph, we have still to answer, What are these particles? Are they vegetable or animal organisms of definite structure, descended from a lineal parentage, and propagating similar or dissimilar offsprings? Or, are they mere simple colloidal structureless atoms—a "leperous distilment" from morbid glands, arising *de novo* under certain conditions? Supposing we give an affirmative to these questions—conceding that some are animals, some vegetables, and some formless albuminous units, of biogenetic and abiogenetic origin—then we come to water, which seems indeed to have no bottom. How is it that they cause so many distinct specific diseases? How is it that they cause disease at all? How is it that, when they have caused one disease (say typhus), it cannot generally be evoked again in the same organism? How is it that they multiply so enormously in an infected organism? How is it that the specific phenomena of all the zymotica may arise successively in the same individual? And how is it that vaccinia is antagonistic to variola?

We now come to consider the 3rd part, viz.:—

Is Milk a Congenial Soil for the Propagation of Zymotic Poison?—Liebig was the first to show that all ferments possess one character in common, viz.—they all contain nitrogen, but there are certain bodies which contain nitrogen and yet are not ferments, as ammonia and hydrocyanic acid. Casein, however, acts as a ferment, as already stated, on the sugar of milk. Casein, also, in the form of pure curd, if exposed to the air,

soon putrefies, and in this state has also the property of inducing a similar condition in other nitrogenous bodies. The casein acts on them as other such bodies had acted on it. The *rationale* of this process is "a transformation or new arrangement of the atoms of a compound yielding two or more new groups, and caused by contact with other substances, the elementary particles of which are themselves in a state of transformation or decomposition. It is a communication or an imparting of a state of motion which the atoms of a body in a state of motion are capable of producing in other bodies whose elementary particles are held together only by a feeble attraction."

It is well known how the mammalian offspring, though receiving no other nourishment than the mother's milk, rapidly increases in weight during the first few months after birth. This proves that milk contains all the elements necessary for building up the tissues of the young animal, and so it does, and that these must also be present in proper proportions and in suitable combinations, and so they are, to be easily assimilated by the tender organs. Milk, then, is simply a solution of the young animal's body as regards the identity of all their chemical elements, and also with reference to most of their chemical compounds. If the foregoing be postulated, it follows that, just as a child or adult is susceptible to be poisoned with small-pox, scarlatinal, or typhoid virus, so also, and in a much higher degree, must milk when exposed to the same influence. Because the living organism has vital force wherewith to oppose the attacks of toxic germs; and just in proportion to the great or little amount of that force contained in the organism will it be able entirely to withstand, or, being infected, successfully to cope with them. But milk is dead organic matter, and having no *vis vitæ*, must be a much more favourable *nidus* for the nurture of contagia than even a sickly organism. "That milk has also the peculiar property of imbibing various effluvia to a great extent, is well known to those accustomed to dairy management. There are many articles liable to impart a taste and odour to milk; boiled fish is particularly obnoxious to the dairy-maid as being sure to taste the cream and hasten decomposition of the milk. Any meat the least tainted will do the same. Onions, even from another room, will give their taste to milk. So, also, will paraffin, turpentine from paint, and varnishes, the smoke of tobacco, and the smell of lime from plaster or whitewash."

Furthermore, the additional physical property of fluidity possessed by milk greatly increases its fertility as a zymotic soil. It is familiar knowledge that the presence of moisture is as necessary in fermentation and putrefaction as the presence

of nitrogen. This is well exemplified in many processes for the preservation of animal tissues by the freezing or exclusion of their water, and it is well known that water is a favourite *habitat* of cholera and typhoid poison.

Milk may thus be looked on as a highly favourable soil for the propagation of contagious germs—1st, In respect of the organic matter it contains; and, 2nd, in regard to its consisting largely of water.

How may milk get Infected with Zymotic Poison?—It may get infected—1st, by impure water; and 2nd, by impure air. For many years past, large quantities of milk have been sent from near and distant farms to Glasgow and other large towns. In these farm-yards the manure—human and bovine—is commonly placed in the most convenient situation for deposit and removal. Generally, at a short distance from the manure heap stands the water-pump. This dangerous juxta-position of ingesta and excreta might not be of much consequence were regard had to the "dip" of the underlying strata (which in Scotland, at least, is seldom horizontal) in determining the safest hygienic position of the well and the "midden." But as such is not the case, it must frequently happen by chance that the manure is placed exactly so that the fluid which percolates through it will gravitate into the well. Doubtless, however, in some cases, there may be a substratum of brick clay impervious to liquids, or a deep and porous subsoil which will have the effect for a time of oxidizing the organic matter during its passage through it, as in sewage irrigation, and thus to a trifling extent render the liquid innocuous. But this process cannot be permanent, as the best-constructed filters for water-works in course of time have their interstices choked with filtered matter, and require renewal. Many must remember how, previous to the introduction of Loch Katrine water into Glasgow, the water then supplied was frequently muddled with a conglomeration of alluvial debris. Hence it is easily seen how the water in farm wells may get seriously contaminated with putrid organic matter. This is more likely to take place during a drought, as then the wells frequently get dry or nearly so, and the proportion of organic matter to the water must be much higher than when the well is full; excepting, perhaps, at the beginning of wet weather, which has been preceded by a drought, when the amount of decayed material washed into the soil is greatest. It is unecessary to say more on this point, as all cases, and they are numerous, of tainted water supply are traceable to one cause, viz., the infiltration of organic matter, call it liquid manure, human excreta, or sewage, through some defect in the drains, or from a want of drains, or from the relative situations of the polluting and polluted substances.

A very important question here presents itself in reference to the origin of, at least, enteric fever—namely, whether in order to produce this fever, it is or is not necessary that there must be present the specific excrement of a previous case of enteric fever. That this fever is engendered by its own excremental pollution of air and drinking water is an established fact, but, as I have previously stated, many distinguished physicians are of opinion that it may arise *de novo*. Dr. Murchison cites numerous instances of sporadic and limited epidemic outbreaks of enteric fever, the spontaneous origin of which in sewer emanation, and in contaminated drinking water, was proved to a certainty—there being no proof of previous pollution with enteric excreta. "This," he says, "explains why fever arising from sewer emanation is always enteric and not typhus or relapsing fever; why enteric fever is endemic in many places and epidemic in circumscribed localities; why it attacks the rich as well as the poor; why it occurs in isolated country houses as well as in large towns; and why it is most prevalent in autumn and warm seasons." In the eighth annual report of the sanitary commissioners of the Government of India, recently issued by Dr. Bryden, the following occurs:—"What I have shown is, that the spontaneous origin of enteric fever is a fact; but that a zymotic element is generated when typhoid is so developed, and that every case of enteric fever should be looked on as a focus from which enteric fever may spread." Premising, then, that typhoid may be elaborated *de novo* in the manner stated, we see that the peculiar conditions favourable to its origin and evolution exist permanently at the very source of a considerable portion of our milk supply, and that, independent of any cases which may occur in the farm-houses themselves. In such an event it is obvious that the danger of infection would be greatly aggravated from the presence of enteric excrement, which all admit contains the special poison in its most virulent form.

It is thus easy to see how the milk may get contaminated with enteric virus. Water from a tainted well may be added to the milk intentionally. But, from all we know of zymotic poisons, a small quantity of such water left in the vessels after washing may be sufficient to infect the milk. In Dr. Ballard's report on the outbreak of enteric fever at Islington—causing 167 cases with 25 deaths, and one at Leeds causing 107 cases with 11 deaths, both traced to the use of impure milk—he has clearly shown that the Leeds outbreak was caused by tainted water mixed with the milk, because, when the handle of the pump which supplied the water was tied, the fever ceased. In the Islington outbreak the balance of evidence points to the milk having been infected by the small quantity of foul water

left in the vessels after washing. Dr. Ballard says—"I confess that to my mind this is not an impossibility; we all know how small, almost infinitesimal, an admixture of sewage will poison a well or stream. Nor is the idea of reproduction of the typhoid contagium out of, within, or in presence of an appropriate organic material, at all foreign to the prevailing opinion upon the subject. Future experience may show that milk, which has remarkable relations to chemical ferments, is a substance peculiarly adapted also to the reproduction of morbid contagia, or to the contagia of typhoid in particular."

(2.) *By impure air.*—That milk, by exposure to air, suspending certain specific contagia, is capable, for a time at least, of preserving, probably of reproducing, whatever portions of the contagia may settle on its surface, either from the air, or from the bodies of infected persons, so that these may evolve their specific morbid phenomena in individuals swallowing the milk, seems to me sufficiently proven by the outbreak of enteric fever at Parkhead, by the second outbreak of enteric fever at Leeds, by the outbreak of enteric fever at Penrith in 1857, by the outbreak of scarlatina at St. Andrews, and also the one of scarlatina at Leeds. My excellent and esteemed friend, Dr. Russell, officer of health for the city, clearly demonstrated the foregoing in his report on the Parkhead outbreak. Many eminent authorities are of opinion that enteric fever is not infectious in the sense that small-pox is. Others hold that the alvine dejections constitute the chief, if not the sole medium of communication. Be that as it may, Dr. Russell has proven (I need not go into details, his report is so recent), that individuals may contract the fever who have not inhaled the emanations directly from enteric excreta,—who have not been exposed to the exhalations of an enteric patient's lungs or skin,—who have not swallowed enteric water, nor respired enteric sewer gas, but, who have partaken of milk rendered zymotic by exposure in the vicinity of a typhoid patient. Of the Leeds outbreak, Dr. Robinson, the officer of health, gives, in his last annual report (1873), the following remarkable details which I have abridged:—"A farm-house in the country became infected with typhoid fever in September last. The head of the house died, and subsequently five members of the household suffered from the disease. Milk, from this infected source, is supplied by a dairyman to a respectable district of the town, where a virulent outbreak of typhoid takes place, and eighty persons who obtained their milk from this dairyman contracted fever, fourteen of whom died. No fever existed at the milk-vendor's house when the cause of the outbreak was found (though subsequently his daughter took typhoid). The farm is visited, the water is found blameless,

being obtained from a source at a higher level than the house, and analysis showed it pure. In the house itself the sick were placed in a room communicating with the kitchen and dairy. Those engaged with the milk attended the sick. Unclean linen was observed placed side by side with the lids of the milk-cans. Outside, an immense fluid reeking mass of manure adjoined the stable where the cows were milked, on to the surface of which the drain from the kitchen sink and slope stone poured its contents. The common privy, which received the typhoid excreta, was overflowing and draining. Thus innumerable particles of poison must have been floating around the house, and from their close contiguity, permeating the cow stable, in which vicinity the milk remained till sent away." This, Dr. Robinson thinks, is sufficient to show not only the possibility, but the almost certain probability of the sad results which followed. It seems to me worthy of remark that Dr. Robinson found those who habitually took milk, or large quantities of cream in their tea or coffee, to suffer from the fever. This proves what I have before stated, that cream is a suitable nidus for zymotic poison, and also that the poison is not destroyed by the usual temperature of hot infusions of tea or coffee. I would further remark that it is highly probable the cause of the fever breaking out in the farm-house originated in the polluted conditon of the farm-yard.

It is, however, obvious that the mere proximity of milk to ærial contagia, or mixture with a small quantity of water impregnated with the poison, might be of little consequence, were it not that it constitutes a congenial soil for their multiplication. Admit this proposition, and the rest follows. Every medical practitioner knows, and can furnish ample proof, how inconceivably subtle contagious or infectious media are. I remember once visiting a young girl residing with her parents in the classic grounds of Ballochmyle. I found her suffering from scarlet-fever. Of all the spots on earth one would have thought this the last in which contagion should be lurking, and the question was, How did she get it? She got it thus:—Workmen were repairing the mansion-house. One had a boy whom I had recently attended for scarlet fever. This boy one day brought his father's dinner from Catrine, about half-a-milé distant. The girl and boy, previously acquaint, happened to meet. They amused themselves a short time on the lawn, and about a week after she took the fever, and gave it also to her brother.

If it be postulated that milk is a congenial zymotic soil, we know that, like all such soils, it will be sure to produce a very large crop from a very small quantity of seed. Hence our care should be to keep the seed from the soil, and the soil from

the seed. Practically, however, this, though comparatively easy in farm-houses, must be a work of difficulty in large towns. In Glasgow, for example, as Dr. Russell has said, "It is very common for parties to live and rear families in rooms behind shops through which the sole access lies, and in which groceries, milk, provisions, fruits, sweetmeats, &c., are sold; and when infectious disease enter such a family it cannot fail to be the source of quite peculiar risk to the public." I need hardly say that I unhesitatingly endorse this statement, and may add that many of the articles sold in such places—as ham, cheese, fish, &c.—from their composition are likely to form suitable zymotic *habitats*, though in some cases the poison may be destroyed by subsequent cooking. At any-rate, it has been shown that milk—which, of course, requires no cooking—is favourable to the preservation, and doubtless also to the reproduction, of enteric, scarlatinal, and I think we may safely include choleraic contagia; such being the case it is extremely probable, in fact I should say certain, that it is also as favourable to the preservation and reproduction of the individual poisons of measles, small-pox, typhus, &c.

To isolate the specific principle of contagia synthetically or analytically, to study their natural history, their physical properties, their chemical affinities, to discover suitable preventives and sure antidotes, is a task which many have undertaken, but hitherto have almost entirely failed in accomplishing. Besides being of absorbing and unflagging interest to the hygienist, biologist, and pathologist, it is one of immense importance to the human race. In England alone the zymotica cause above 100,000 deaths annually, and though their excessively attenuated atoms have hitherto practically eluded the most refined methods of research, failing to be ensnared by the most subtle and delicately-contrived experiments, still I cannot but think that, with higher microscopical powers, greater facilities for original experimental investigation, increased and more exact knowledge of physiological and pathological chemistry, assisted by fresh discoveries in the various allied sciences, their total subjugation to the will of man is simply a question of time.—*Glasgow Medical Journal*, *May* 1873, *p.* 312.

15.—ON THE ORIGIN OF TYPHOID BY INFECTED MILK.

By the EDITOR OF THE LANCET.

In illustration of the milk-theory of the origin of typhoid fever, Mr. Alfred Smee communicated to the *Times* some observations which, though not entirely novel, are of very great importance. Mr. Smee's London house is, it appears,

supplied with milk, cream, and butter from some cows of his own at Wallington. For a short time in the spring these cows were fed on sewage grass, when the butter became so offensively rancid that the family could not bear it on the table. When the sewage was discontinued, "the cream, milk, and butter resumed their former excellence." The experiment was repeated without notice to the family, with the same result as before; and Mr. Smee declares that he has reason to suspect that the fact "is thoroughly well known to our dairy proprietors." Mr. Smee introduces his remarks with the succinct statement that he is "in a position to explain the occurrence of typhoid fever from the use of milk."

It will be familiar to many of our readers that fears have frequently been expressed of the effect of sewage grass on the lower animals, and so, indirectly, on man. The point has been considered by almost every one of the commissions and committees which have investigated the sewage question; and we believe we are correct in stating that in every case the result has been a conviction in the minds of the reporters that these fears were illusory, and that, practically, there was no more danger in applying sewage to grass than in applying the liquid manure of a farm-yard to a growing crop. We are by no means anxious to overrate these conclusions. The observations upon which they rest are not sufficiently numerous or exact to justify anything like dogmatism in regard to them. It is proverbially difficult to prove a negative, and the question is one of such vast importance that any statements bearing upon it are entitled to the most respectful consideration. But, nevertheless, the fact remains that to the present time no distinct evidence has been brought forward of evil arising from sewage irrigation; and when we consider that irrigation is pretty generally allowed to be on the whole the most satisfactory method for the disposal of sewage, we find ourselves compelled to ask for very distinct evidence of its danger before we venture to discard it.

The oldest and in some respects the most remarkable example of British sewage irrigation is afforded by the well known Craigentinny and other meadows near Edinburgh, where four or five good grass crops are yielded every year by land which was formerly barren sand, and where the system has been in partial use for about two hundred years. In these meadows the grass receives an enormous and excessive amount of sewage, complete purification being unnecessary, as the effluent water goes to the sea. The remark made in regard to them by the Rivers' Pollution Commissioners has therefore considerable force, and may well be quoted here: "If sewage irrigation had really bred a special class of diseases, it would hardly

have been tolerated for two centuries on so vast a scale near Edinburgh."

Even more satisfactory, because more systematically and scientifically observed, are the large experiments on irrigation which have for years been conducted at Rugby and Croydon, those at Rugby having been framed and carried out by the scientific members of a Royal Commission. No grave objection was urged against these experiments by anyone. At Rugby cattle were fed extensively and continuously with the sewage grass, and the milk was regularly analysed for two years. No important difference was observed between the milk from sewaged and unsewaged grass, and it is scarcely possible that any could have existed, as it could hardly have escaped the notice of such experienced and impartial observers as Professor Way and Dr. Gilbert.

We will turn now to another point of evidence. Some time ago the Sewage Committee of the British Association commissioned Dr. Spencer Cobbold "to examine and report as to the presence of parasites in the flesh of a cow which was reared and fattened exclusively on the products of sewage irrigation by W. Hope, Esq., V.C., of Breton's farm." Dr. Cobbold evidently held a strong feeling of the danger of sewage irrigation, but nevertheless he found "that the animal in question was entirely free from internal parasites of any kind," This observation, though on a different point to that now under discussion, has great value as showing that the cow, at any rate, is not injuriously affected by sewage grass.

Since the publication of Mr. Smee's letter, several letters from medical officers, managers of lunatic asylums, and others who have had experience in sewage irrigation, have appeared in the *Times*. They all agree that no harm is done to the milk by the use of sewage grass. The South Metropolitan School may be taken as an example. The population in these schools is sixteen hundred, and the daily consumption of water twenty thousand gallons, the whole of the sewage being applied to land. The cows eat the grass with relish; and Mr. Hales tells us that, after an experience of four years, he "can assert with much confidence that no evil has arisen from the use of this grass."

No doubt sewage irrigation requires great care and skill in its management; and it may be conceded that even in the most carefully regulated sewage farms noxious emanations cannot be entirely avoided.

Sewage irrigation is at the best an imperfect solution of the great problem, and the greatest praise that can be accorded to it is that it appears preferable to any of the schemes which have been proposed in its place. Perhaps the best statement

of the objections to it is to be found in a recent report by Herr Lefeldt, a civil engineer who was appointed by the Prussian Minister of Agriculture to investigate the various English systems for the disposal of sewage. A remark which he makes with regard to the grass of the Edinburgh meadows may be quoted as lending some support to the theory adopted by Mr. Smee. He says: "The capillary tubes for some inches above the roots are found to be filled with unassimilated fæcal matters;" and adds that, if the irrigation is continued till within two days of the mowing, the cattle will not eat the grass. Here clearly is a source of danger; but it must not be forgotten that in these meadows the sewage is not so much applied as misapplied to the grass.

In conclusion we must remark that, while the evidence of the sanitary safety of sewage irrigation is less perfect than it should be, we decline to throw it all on one side for the sake of the still more imperfect, and, as far as we know, isolated evidence adduced by Mr. Smee.—*Lancet, Aug.* 23, 1873, *p.* 271.

16.—AFFECTABLE AND INFECTABLE SUSCEPTIBILITY OF MILK.

By Dr. H. Willington.

[Some years ago the family of the writer of this paper suffered from an unaccountable series of gastric attacks, sometimes typhoid, and once attended with malignant thrush. The notes made at the time have unfortunately been lost.]

It is something like fourteen years since my attention was painfully called to the putrid element to be met with too often in London milk. My own senses of smell and taste are, unfortunately, morbidly great for my comfort. To this I attributed the puking sensation arising instantly I smelt some London milk. In consequence of this, and of other members of my family not perceiving the mawkish sickening odour I did, I willingly attributed my revulsions of taste and smell to my own morbidness, until on one occasion of meeting, in the mawkish odour, with a distinct evidence of putrescent odour, I I was induced to more carefully inquire into the facts—firstly, as to the cleanliness on the part of our servants and of the jugs used, and then as to the condition of the milk as it came from the "milkman." The milkman delivered the milk possessed with the odour of putrescence, the milk itself presenting no unnatural look, yielding also a fair result to the lactometer. The milkman had the milk from his own cows, pent in a small crowded shed. I accused them of possible scrofula, and ceased to have their milk. My family were sent to the seaside and

recovered health, the incidence of causation of their gastric attacks and fevers being forgotten or given up.

Some time afterwards I was called to the house of the milk-vendor to a case of sudden illness, from faintness and gastric pains, so far as I now recollect. The odour of the house was such as follows a day after a cesspool has been emptied. I complained, "It was quite poisonous," to which was answered, "It was dreadful—from the drains; but the landlord would do nothing, and they could not. They were sure it was not from want of washing, &c." This seemed correct, if appearances might judge the case. I was struck by the appearance of eight or nine open pans of milk standing on chairs; so mawkish the odour that I could not tolerate it. The sink was open. Their water-supply from the waterworks. The house was drained into a cesspool to which the sink had no alternative but go. The drain through the house, as was subsequently proved, was choked—eighteen feet full length—and was virtually eighteen feet of superficial cesspool. I arrived at the conclusion that the milk was affected by the odorous poison of the atmosphere, wherein lay the cause of the putrescent odour of the milk that had entered my nursery when I was supplied by this dairy.

The short street this house was in was proverbial for fevers, and every epidemic that showed up in season. The drainage of it, after much trouble, was improved, fevers ceased in great measure, and my inquiry into the milk as a disseminator ceased after I had done the best I was able in ascertaining the affectability of milk—under given circumstances of temperature—by putrescent odours.

A little was exposed in a saucer all night near a sink that had the trap removed. In the morning an odour mawkish and unnatural to milk was discovered by my sense of smell, but others thought it was my fancy.

The late outbreak of fever through infected milk has aroused the slumbering theory—Milk is an affectable body, and, being an animal substance or product, is essentially susceptible of affection for putrefying causes, like as veal is, and especially so when there is a thundery atmosphere. That milk is attractive of electrical conditions, or, if it be preferred, is susceptible to them, is too patent to need the assertion they are so; and so is veal—a meat fed on milk—remarkably so, and to the extent of utter decomposition of the fluids of the meat in a few hours, causing an attraction to flies that collects them from all parts of the place. Taking all these things into account, it has appeared to me a most feasible conclusion: Milk is affectable by atmospheric poison, and is capable of incubating the worst morbid principle, and of becoming thereby an infectant of those who drink it.

It requires much experiment to prove putrid water, unless it has had such time as never is possible between milking the milk and the sale and use of it. Was it the infectious element *per se* that affected the milk which affected the Marylebone families?

The temperature of newly-drawn milk, given certain atmospheric conditions and sewage odour, favours its affectability by a tainted atmosphere, especially if the latter come up in volume from an open sink into the dairy in the night time.

If zymotic causes follow currents and deposits, where susceptibility is suitable, in tanks of water, why not on open dairy milk-pans of milk, if the dairy be close to open cesspools the holes and depressions are made into in every farmyard?

Farmers for the most part, if they live long, are windy or pot-bellied—a fact I have long since attributed to gastric poisoning by the open cesspool system of their premises. Again, when the hot season comes the supply of water is reduced, and cows are driven to drink from ponds that are little less than sewage or bilge-water reservoirs.—*Lancet, August* 23, 1873, *p.* 284.

17.—THE ELECTROLYTIC TREATMENT OF CANCER.

By M. NEFTEL.

This paper contains the description of a mode of treatment which, if experience confirms the results stated here, must be considered one of the greatest boons which have been conferred on suffering humanity. It is no less than a cure for cancer. The paper begins with a discussion as to the nature of cancer. Following the school in which he was educated, the author at first looked on malignant disease as primarily a constitutional affection, of which the tumour was merely the local manifestation. He has been, however, convinced, by the arguments of Virchow, that cancer is really at first a purely local disease, and that any generalisation which may occur is a secondary result of the infection of the system from the local disease. He treats of the traumatic origin of cancer, and gives two cases in which mammary scirrhus was distinctly traced to the effects of a blow. Coming to treatment, excision is generally of little or no advantage. He has found electrolysis alone useful. Before giving the results of his treatment and the method, he discusses its *modus operandi;* and states it as his belief that the action is not that of a caustic; but that the electric stimulation has some influence on the protoplasm of the cancer-cells, causing them to alter their mode of life. He believes in Waldeyer's views as to the development of cancer, namely, that it always

originates from epithelium, and does not distinguish clinically between carcinoma and cancroid. The first case in which he tried the treatment is given in detail, and is of considerable interest. A gentleman had a tumour of the left mamma, with secondary affection of the axillary glands. It was diagnosed as cancer by Nelaton, Moore, Marion Sims, &c., and was finally extirpated. A similar tumour appeared on the right breast; while the glandular enlargements in the left axilla recurred, and the patient became markedly cachectic. This tumour in the right breast, as well as the axillary glands, were treated in the manner to be afterwards described; and the result was that the tumour completely disappeared, and the cachexia was entirely recovered from. A variety of other cases are given, all showing how speedily many cancers can be made to disappear, and especially how, after the first electrolysis, the lancinating pain passes off. In accordance with Virchow's views, he believes that there is an initial stage in which cancer is purely local, and in this stage he asserts that it can be completely cured by electrolysis. When the disease has advanced beyond this the cure is difficult, and its success depends on whether all the affected parts can be reached. When the tumour is adherent to the deeper parts, or when distant organs are secondarily affected, the treatment is generally unsuccessful. Even in the most aggravated cases, however, this is the best means of reducing the pain, and improving the general condition of the patient. The method of applying the agent is described with great care. Suppose, in the first place, that the tumour is of moderate size, hard, and slow of growth. With or without chloroform, as circumstances may determine, two, three, or four needles are introduced at equal distances into the tumour, and connected with the negative pole or cathode of the battery. The positive pole or anode is connected with a broad plate, which is applied with a wet compress to the skin at a certain distance from the tumour, and moved from place to place. The current is gradually increased up to thirty-five or forty elements, and after twenty to thirty minutes, gradually lessened, and needle after needle withdrawn. This operation is repeated for a limited number of times at intervals of three or four days, the needles being introduced at different parts of the tumour. After this energetic treatment, a weak current of four to eight elements is to be used daily for months. The negative pole, formed of a flat plate and a wet compress, is placed over the needle punctures, and the positive at a distance from the tumour as before. The sloughs produced by the needles come away after a time, and their track serves as a conductor for the current. If, in the second place, the tumour be a large one, then the only modification is that, during the

operation, needles should be extracted and placed in different situations. The number of the needles should not, however, be increased above four at one time. If, in the third place, the tumour be large and ulcerating, then an attempt is made to produce sloughing of it. In this case, the anode (or positive) needle is thrust into the centre of the tumour, and its base transfixed with a number of needles connected with the negative pole. These are left for about twenty minutes, and then inserted in a different position. In from seven to ten days the whole or part of the tumour generally sloughs away, and if it is not entirely removed, the operation may be repeated. After this rather severe treatment, there is never any fever or special constitutional disturbance. In this case, also, after the tumour has sloughed, the treatment with weak currents should be continued for months. Of late, the author has sometimes introduced a slight difference in the treatment of the first and second class of cases. Instead of inserting cathode needles throughout, he has occasionally begun with an anode needle, which he has only kept in for a short time, and he thinks this an improvement on the original plan. The anode needle should only be kept in for a short time, as otherwise it is apt to cause sloughing; after this single application of the positive needle, he continues as before. It is to be noted that the author attaches great importance to the efficient carrying out of the treatment with weak currents, after the energetic currents have been used preliminarily; and he generally entrusts this latter treatment to the patient. The author has also used a similar mode of treatment, as others have done, in aneurisms, varix, &c. His method is in this respect different from the usual, that he does not insulate the stems of the needles which he introduces.—*Edinburgh Medical Journal, Oct.* 1873, *p.* 381.

18.—THE CO-EXISTENCE OF CARBUNCLE AND GOUT.

By Dr. J. M. WINN, London.

[A correspondent of the Lancet (Dr. Ellery) having asked whether any member of the profession had ever noticed a connexion between gout and carbuncle, Dr. Winn in reply refers him to an essay on Hereditary Diseases, which he published in 1869, and in which he states, in reference to the correlation of force in disease:]

For many years I have been struck with the frequency of the occurrence of carbuncle in individuals of a gouty diathesis. The worst case of carbuncle I ever met with was that of an individual who had suffered from repeated attacks of podagra. The usual symptoms of gout had, however, been for some time

in abeyance before the commencement of his fatal illness; and the carbuncular disease of which he died was evidently the vicarious action of the same subtle morbific force as that which had previously caused his sufferings from gout.—*Lancet*, *May* 3, 1873, *p.* 653.

[There is another connexion which is perhaps more interesting that this, and is not, we believe, known to the profession—viz., that in carbuncle there is generally temporary diabetes.—EDS.]

19.—ON THE INTERNAL USE OF CHLORALUM IN CHOLERA.

By Dr. HENRY BLANC, F.R.G.S., Surgeon H.M. Indian Army.

During the months of May, June, and July, 1872, cholera was raging all over the Mahratta country (the plateau above the Western Ghauts, Bombay Presidency), and at Sattara, where I was acting civil surgeon. I had occasion to see a few cases. We did not have many, but the cholera cannot be blamed for having treated us so leniently, and doubtless we would have numbered the victims of the epidemic by hundreds, instead of by tens, had I not been most cordially assisted by the civil officers in charge of the district. Thanks to them, the sanitary measures I recommended were carried out most vigorously, and the consequence was that cholera was stamped out before it could get a footing; moreover, we succeeded in protecting entirely the camp from its influence, although the military station is situate less than a mile from the town of Sattara. I do not intend recording here the hygienic measures which saved us from the heavy death-bill, because, if they are not more generally applied, they are, nevertheless, perfectly known. I wish merely to say a few words on the treatment I adopted. The type of the disease was most virulent. Generally no premonitory symptoms, hardly any vomiting or purging; persons in perfect health, after vomiting once or twice, were at once seized with rapid and deep collapse.

I tried every possible remedy, from the orthodox cholera mixture and pills to the injection of liquor ammoniæ in the veins. The hypodermic injection of morphia, recommended by a writer in the Lancet as having given good results at Constantinople during the epidemic that prevailed there, totally failed. Salt-and-water enemas and exhilarant stimulants were also unsuccessful.

"*En desespoir de cause*," I thought of destroying the poison in the body itself by means of some powerful disinfectant,

believing that if this were possible, we ought, in many cases apparently hopeless, to save life, and, once the cause removed, by merely directing and assisting nature, obtain a good per centage of successes. I selected chloralum—the chloride of aluminium—not only on account of its freedom from smell and taste, but also because it could not, diluted with water, do harm if it did no good.

I made a mixture of one part of liquid chloralum to twenty-five of water, and directed that an ounce of the mixture be given every half hour, and that an enema, containing sixteen ounces of the mixture, be administered every hour. Undiluted chloralum was to be sprinkled frequently on and around the bed. In the two successful cases, the remedy was given less and less frequently, as the reaction became more completely established.

I can only regret that I did not think of the chloralum mixture sooner, as it is difficult to judge of the value of a remedy on such a limited number of cases. Still it should be remembered that it was employed in *very desperate cases*, quite as unfavourable as those previously treated by various other means, and which had all of them ended fatally. Theoretically we have a better chance of success with an antidote than by the employment of drugs, which are directed merely against the symptoms of the disease. We know that opium and alcoholic stimulants do much harm, and as for purgatives—castor oil or calomel—they are of more use in the druggist's bottle than in the stomach of an individual rapidly sinking under the deep collapse of a severe attack of cholera. I am far from asserting that we have in chloralum "the remedy" for cholera; still, should this dreadful scourge visit the shores of England, there can be no objection, in presence of the cases here recorded, to give the remedy a fair trial.

[A tabular summary of three cases treated is given.]

The second case given above, and which was followed by death, should not be considered as an unsuccess. The patient refused in my presence to take any European medicine, and it was only after much coaxing that he swallowed a dose. Doubtless, after my departure, he was allowed by the native medical subordinates to have his own way. In order to ascertain the real value of chloralum in cholera, it should be given before other medicines have been administered. In cholera we have but too often to contend against two evils, the disease itself, and the previous indiscriminate use of a host of remedies, most of them, if not all, adding their noxious influences to the many difficulties we have already to contend with.—*Lancet, Aug.* 16, 1873, *p.* 226.

DISEASES OF THE NERVOUS SYSTEM.

20.—ARE MORBID FUNCTIONAL STATES OF THE CEREBELLUM, MEDULLA OBLONGATA, AND SPINAL CORD, CAUSES OF ALBUMINURIA AND BRIGHT'S DISEASE?

By Dr. THOMAS LAYCOCK, Professor of the Practice of Medicine and of Clinical Medicine in the University of Edinburgh.

A year ago an interesting polemic arose in the Royal Medico-Chirurgical Society as to the nature and causes of certain pathological changes in the minute arteries of the body in general, and in the kidneys in especial, found in cases of chronic Bright's disease. Differences of opinion were expressed by eminent physicians whose views will be always welcomed by the profession, both as to the facts and the theories involved. A committee of the society was appointed to judge between them, but it resigned office, and the whole matter is still undetermined.

Looking at the questions in dispute as a spectator anxious to derive useful knowledge from the very vivid discussions at the society, and in the lectures subsequently published, to elucidate and round it off, I find the controversialists agreed as to this general fact of observation—that there is a change in the structure of the smaller arteries throughout the body generally, as well as in the kidneys, in a certain class of cases of Bright's disease. The difference in opinion was as to the nature of the change. Sir William Gull and Dr. Sutton reported it to be a hyaline-fibroid or fibroid thickening of the arterial wall, whereas Dr. Johnson held that it was a muscular hypertrophy. The obvious method of solution of this part of the question is that each observer should prepare demonstrations from the same subject, for if they be from different subjects it is quite probable that in the one may be hyaline-fibroid and in the other hypertrophic. In the absence of this procedure, it is fair to conclude at least that both observers may be correct.

But the polemic extended far beyond the anatomical facts to the causes of the changes observed. Dr. Geo. Johnson is an earnest humoral pathologist, and affirms that Bright's disease is not a mere local malady, and that "the proximate cause is in all probability a morbid condition of the blood." In proof, he observes that the disease is bilateral, and that it "occurs in association with morbid states in which a morbid condition of the blood may be confidently assumed to exist. Albuminuria, varying in degree and in duration, has been found more or less frequently associated with scarlet fever, measles, small-pox,

erysipelas, pyæmia, typhus and typhoid fever, rheumatic fever, malarious fevers, cholera, purpura, scurvy, diabetes, syphilis, certain forms of pneumonia, pregnancy, the absorption of secretions from the interior of the uterus after parturition, gout, the abuse of alcoholic liquors, excessive eating, certain forms of dyspepsia—resulting, as may be supposed, in the passage of crude materials into the circulation,—a poor and insufficient diet, purulent and other discharges, and, lastly, suppressed action of the skin by exposure to cold, and especially to cold and wet combined."

It will be observed that in this copious exposition of causation, albumen in the urine, or "albuminuria," is taken as identical with, or necessarily leading to, Bright's disease. Further, that while in all these varied diseases the nervous system as well as the blood must be involved—in some more primarily than in others—it is wholly ignored in causation. And exciting causes of albuminuria traceable directly to the nervous system—viz., that kind following upon excessive brain work, and due to lesions of the medulla oblongata and spinal cord—are omitted. In short, I do not find in Dr. Johnson's papers any reference whatever to any morbid state of the nervous system in general, or that of the kidneys in especial, as an antecedent to the morbid changes in the kidneys or to those of the blood, which are supposed, "in all probability, to be their cause."

In reference to this omission of an important element in causation, I may also refer to the recent discussion as to influence of "alcoholic" drinks on the production of Bright's disease. Neither Dr. Johnson nor the other disputants seem to realise the fact that the drinks of the drunkard are not all "alcoholic" in a definite meaning of the term, but contain other agents of a poisonous or injurious character, as fusel oil, bitters, &c., and that these have all a direct and primary action on the nervous system, and so their use leads to morbid conditions thereof which may be predisposing causes. Lead is another of these neurotic causes. What Dr. Johnson dwells upon is the series of diseases of the nervous system which coincide with, or are consecutive to, chronic Bright's disease. but which, according to his humoral pathology, also depend on a poisoned state of the blood, poisoned by the retention of urinary excreta, and known by the term "uræmia." Yet he states a well-known clinical fact, which is of itself sufficient to upset the current theories of uræmic poisoning. "Let me add," he observes in one of his lectures, "that in some cases, notwithstanding the scantiness, and ultimately the almost complete suppression, of urine, uræmic symptoms are almost entirely wanting.

But how, according to the humoral theory, is the arterial degeneration or hypertrophy of the small arteries generally caused? and how the coincident hypertrophy of the heart, noticed by Bright? "About six years ago it occurred to me," Dr. Johnson observes, "that the hypertrophy of the left ventricle of the heart in cases of contracted kidney might be a result of increased contraction of the small arteries throughout the body, this contraction being excited by the abnormal quality of the blood." And this kind of arterial change is also the cause of the neuroses in certain cases of Bright's disease—excessive contraction of the arteries of nerve-centres, "excited by the pressure of impure blood," leading to arrest of supply of blood to the nerve-tissue. Here, again, it is observable how entirely the vaso-motor nervous system is ignored. Sir William Gull differs wholly, and I think rightly, from Dr. Johnson in respect to his humoral pathology.

For many years past I have directed attention to the influential part which the nervous system plays in all these tissue changes whatever, showing the facts both systematically in my college course and at the bedside. I have drawn the conclusion from a large and varied series of facts, that the cerebellum, with its connexion downwards along the medulla oblongata and spinal cord and upwards into the hemispheres, is the great trophic centre; and that this centre is put into functional activity, not only by afferent impressions from the body, whether consciousness be coincident or not, but also by afferent impressions from the vast periphery of the hemispheres, and in this case also whether consciousness be coincident or not;—in short, that the hemispheres are not functionally restricted to intellectual processes, as is generally held, nor the cerebellum to muscular co-ordination, but that both conjointly influence the whole of the organic processes in a way which physiologists and pathologists have, so far as I know, wholly overlooked. It is in accordance with these views that I regard the changes in the vascular system in general described by Dr. Johnson as due to concauses having their seat in the cerebellum, medulla oblongata, and spinal cord; that a special neurosis of the renal nervous system is a concause in all cases of albuminuria and consecutive structural changes in the kidneys; and that the dropsies, thirst, cardiac, vascular, lymphatic, pulmonary, gastric, and other phenomena, must be reconsidered from this point of view to understand in any reasonable degree the true pathology and the prevention and cure of this large and common group of diseases. Hence I have put for consideration the question at the head of this note—Are morbid functional states of the cerebellum, medulla, oblongata, and spinal cord causes, predisposing and exciting,

of albuminuria and Bright's disease?—*Med. Times and Gazette, July* 12, 1873, *p.* 30.

21.—ON PARALYSIS, CONVULSION, AND OTHER NERVOUS AFFECTIONS IN SYPHILITIC SUBJECTS.

By Dr. THOMAS BUZZARD, Physician to the National Hospital for the Paralysed and Epileptic.

Ricord's views as to the most appropriate treatment for the different stages of constitutional syphilis may be thus epitomised. In the primary stage, mercury. For secondary symptoms, when these are uncomplicated, mercury; when they are blended with those of a tertiary character, the treatment is a mixture of mercury and iodide of potassium. In the tertiary stage he relies upon iodide of potassium. And this scheme would probably represent very fairly the views of those who have had large experience of the disease. In the class of cases, however, with which we are specially concerned, as I have before described, it is not by any means always possible to say whether the symptoms of nervous affection which are observed belong to the secondary or tertiary stage. Our treatment therefore must be to a certain extent tentative. My own plan is always to commence, in a case of this kind, with iodide of potassium. In a majority of instances this drug, which will often require to be pushed to large doses, will be sufficient to bring about a cure of the symptoms, or at least a very important improvement. If, however, after three or four weeks of such treatment, employed with sufficient boldness, the improvement is not marked, I give mercury, which will sometimes then act with remarkable effect. The preparations which I chiefly employ are, the perchloride and the red iodide; and I do not know that preference, for any particular reason, is to be given to one or other of these forms. As a matter of practice, I think I have found that the red iodide, given in solution of iodide of potassium, is less apt to cause irritation of the abdominal organs than the perchloride. It is very seldom that either causes salivation. Where it is desired to produce, and to keep up for some time, a slight action upon the gums, this can be effected in the usual manner by appropriate doses of blue pill or grey powder. The French very largely employ the green iodide, a preparation but seldom used in England, probably because it is unstable, some biniodide being apt to form in it when it has been long kept.

A word as to the dose of iodide of potassium. After a good deal of hesitation, and trial of various quantities in a considerable number of cases, I feel convinced that in syphilitic

affections of the nervous system it is often necessary to employ doses of this drug which are far beyond those usually ordered. In several instances I have observed something of the following kind to take place. An improvement up to a certain point has been produced by doses of iodide of from ten or fifteen to twenty grains three times daily. The patient has then remained at this stage, or progress has been very slow, whilst continuing to take this amount. On increasing it, however, by rapid steps to thirty, forty, sixty, or even ninety grains three times a day, the case has responded immediately and *pari passu* to the additional quantity of the drug. There are two patients in the hospital at the present time who are striking instances of this effect. In each the dose was pushed gradually to ninety grains three times a day, with marked beneficial effect, and I may add that the patients themselves express an unhesitating opinion upon this point. There is nothing new of course in the employment of large doses. Forty years ago the late Dr. Elliotson used to give as much as two drachms three times a day, and with remarkably good results. But of late years, as a general rule, the dose has been so moderate that to many practitioners the employment of ten grains at a time is only gradually arrived at, and with some caution. No doubt in a very large number of cases a comparatively small dose is all that is required, and in practice, therefore, it is well to begin with a dose of ten grains, and increase it if necessary. I feel tolerably sure from repeated experiments that the iodide may be used, if occasion require it, as freely as the bromide of potassium, and that the opportunity of doing great good in syphilitic nervous affections is nearly as often missed by the employment of inadequate doses of the former drug as used notoriously to happen in respect to epilepsy from the exhibition of too small doses of the latter. Symptoms of iodism, very like those of severe nasal catarrh, will occasionally present themselves, but just as frequently, so far as I have seen, when the amount of the salt employed is small as when it is considerable, and it is very rare indeed that the use of the drug has to be given up on this account. Occasionally, too, a kind of large pustular acne is a disagreeable consequence of its administration, but this is not so common as where the bromide has been given.

Whether iodide of potassium, or mercury, or both be employed, a very long continuance of the treatment seems always to be required, and an immediate resumption of it at the slightest indication of relapse. In certain cases, arsenic and iron may be used with advantage in addition to these means, bromide of potassium may be added in cases of convulsion, and electricity may be employed to help the nutrition of

wasted muscles; but practically the treatment of this class of nervous affections is the treatment of the constitutional disorder.—*Lancet, May* 17, 1873, *p.* 693.

22.—NOTE ON THE PARALYSING ACTION OF ACONITE ON THE SYMPATHETIC NERVE.

By Dr. FREDERICK BAGSHAWE, M.A., Assistant-Surgeon to the East Sussex and Hastings Infirmary.

The topical action of aconite was displayed in the following case in so marked a manner as to be worth recording.

A young lady, aged nineteen, came under my care in November 1872, who had long been suffering from spinal irritation and neuralgia. During the previous summer she had passed some time at St. Moritz with benefit, and could then walk four or five miles. A recurrence of severe pain, sometimes in the spine, and sometimes in the face, head, and neck, had induced her to seek further change at Hastings. On the 3rd of November, having recovered from an attack of spinal pain, the patient took a short walk while a north-east wind was blowing, and came home with severe pains in the head and neck, chiefly on the left side. The effect of the subcutaneous injection of $\frac{1}{4}$ grain of morphia was to induce sickness, while it failed to relieve pain or to procure sleep. Liniment of chloroform and belladonna was applied, but she passed a disturbed night, and next day was suffering much pain along the course of the left occipito-cervical and trigeminal nerves. I applied the liniment of aconite (B. P.) behind the left ear and down the neck, carefully avoiding a small place which had been blistered by the chloroform liniment. Some relief was obtained, and the liniment was again painted on twice that day (5th Nov.), and produced some numbness and tingling of the lips. On the morning of the 6th more liniment was applied, as this was found to be "the only thing that drove away the pain. At 9 a.m. the eyes and nose felt sore and tingling "as if she had a cold in the head;" at 1 p.m. burning soreness of the eyes was increasing, the skin of the neck was numb, and the lips tingled much; at 3 p.m. she was screening her eyes from the light, but rejoicing in being free from the neuralgia. The left eyelid was much swollen. The vessels of the ocular conjunctiva were enlarged, bulging, and tortuous, and of a bright pink hue. The forehead, face, ear, and neck were hot to the touch, while the right side was cool. The pupil was widely dilated, and vision was greatly impaired, so that she was unable to read good letterpress, but could read large print of a title-page.

The lips were dry. The tongue was projected somewhat to the affected side. This was tried twice. There were no general symptoms, no weakness. The pulse was 88. At 6 p.m. pain in the eyes and intolerance of light were still greater, but she was free from neuralgia and perfectly well, the eyes excepted. The pupil being widely dilated, I examined with the ophthalmoscope, and saw the arteria centralis distended and the retina generally congested. Sensibility, tested by pricking, was greatly impaired over the whole side of the neck and face to which the aconite had been applied.

The symptoms abated at 9 p.m., and next morning (11 a.m.) the congestion of the eye had almost subsided, slight redness and swelling only remaining. The left pupil was still larger than the right; both contracted well to the stimulus of light. There was no return of neuralgia.

It is not uncommon to meet with less marked cases of the effects produced by the topical use of aconite, thus:—

Mr. W. S., aged 50, suffering from facial neuralgia, rubbed aconite on the cheek-bone. It caused numbness and "twittering" sensation of the part, which lasted some hours, while the pain was dulled. There was sensible increase of warmth of the muscles of the face. The eye became weak and watering, while the lid drooped. The state of the pupil was not noted. This condition lasted about twenty-four hours.

Mrs. W. D., aged 57, used aconite liniment with temporary relief to her neuralgia, and with increased warmth to the facial muscles and manifest congestion of the conjunctival vessels.

These and similar cases point to the fact that aconite applied to the skin in the neighbourhood of the cervical ganglia and branches of the sympathetic, produces consequences similar to those brought about by section of the nerve in that situation in animals. In both cases a swollen and congested state of the conjunctival vessels and abnormal warmth of the part is the result.

In both the threefold office ascribed to the sympathetic is interfered with: (1) sensation is blunted in the parts supplied by the nerve; (2) nervous control over the blood-vessels is withdrawn; (3) motive power is reduced.

Aconite thus becomes a powerful local remedy in our hands, not only as a controller of morbid sensation, but as a retarder of vascular action, and by consequence as a modifier of muscular action.

But although generally a painless remedy, it is a safe and innocuous one? Happily, it appears that the small quantities of the drug capable of being absorbed through the skin, attack the nerves and muscles directly in their neighbourhood, and

show little disposition to be absorbed into the general current of the circulation, and so paralyse the great central circulating organ. In the above detailed case the local action of the remedy was powerfully exerted, but neither stomach, kidneys, nor heart seemed to sympathise. None the less, however, is caution needed in the use of so powerful an agent, lest its action should pass on to the cardiac nerves and vessels, and so produce grave symptoms.

The following case of general aconite poisoning illustrates some of these symptoms, and shows at the same time how large an internal dose of aconite may be recovered from. In December 1871 Mrs. G. was attending the Hastings Infirmary as out-patient, suffering from pain in the shoulder, and was ordered a liniment composed of equal parts lin. opii and lin. aconiti (B. P.): she was duly cautioned as to the poisonous nature of the application. On January 5 she drank off about six drachms of the liniment, *i.e.* three drachms of lin. aconiti. Immediately afterwards she felt a burning sensation in the throat and tongue; in about ten minutes she was unable to speak, and is said to have made a stertorous noise in breathing; she suffered from a strangling sensation in the throat. The face was partly pallid, partly flushed (over the cheek-bone). A neighbouring chemist who was called in encouraged vomiting, which had already set in, with mustard emetics, followed by ʒ ss of sulphate of zinc: he did not yet know what poison had been taken. I saw the patient an hour and a half after the dose had been taken; she was then agitated and weak, with a pulse of 84 and a cool skin; her only complaint was of burning at the throat and stomach, and weakness. *The pupils were larger than natural.* She took some brandy and raw egg, and a stimulating draught from time to time. Next day she felt faint and low, and some amount of burning sensation in the throat and stomach continued; the face was flushed, the pulse 80 and good; the urine remained normal, and much milk was secreted. On the 7th she appeared to be well.

In cases of fatal aconite poisoning the symptoms seem to have followed this order:—

1. Tingling and numbness of the tongue and throat, soon extending to the stomach, with local sensations of heat.
2. Failure of the circulation, evidenced by cold extremities, cold sweats, great feebleness, &c.
3. Pupils dilated and sight dimmed.
4. Fatal syncope.

[As a pendant to Dr. Bagshawe's cases of the local action of aconite upon the vaso-motor nerves, I beg to mention the following, which occurred some months ago in the out-patient room:—E. M., a small and thin woman, aged 29, married, but

has never had children: she had had threatenings of phthisis, and had also suffered intercostal neuralgia. Just after Dr. Bagshawe had informed me, verbally, of his first case, this woman applied one day at the hospital, suffering from severe pain in the left trigeminus (first and second division), which was evidently excited by a very bad carious tooth. She would not have the tooth extracted, and, as an experiment upon the powers of aconite, I ordered the lin. aconiti to be painted over the painful parts. The effect in arresting pain was immediate and complete, but on the next hospital day she came complaining of "erysipelas." The parts which had been painted were swollen and bright red; moreover the conjunctiva of that side was severely congested, with *tortuous vessels*. The pupil in this case was contracted rather than dilated; and this leads me to notice the curious fact that in Dr. Bagshawe's cases the pupils were dilated, in his first case to a remarkable extent. I need not say that, while the rest of the phenomena agree well enough with the idea of sympathetic paralysis, dilatation of the pupil is exactly the reverse of the condition of iris produced in Bernard's and Brown-Séquard's experiments.—F. E. ANSTIE.]—*Practitioner*, *July* 1873, *p.* 24.

28.—ON THE ANATOMICAL INVESTIGATION OF EPILEPSY AND EPILEPTIFORM CONVULSIONS.

By Dr. J. HUGHLINGS JACKSON, Physician to the Hospital for the Epileptic and Paralysed, and to the London Hospital.

[In this paper Dr. Jackson pursues an anatomical line of inquiry with the intention of showing the bearings of Ferrier's researches on the methodical study of the seat of the lesion in epilepsies. There are certain principles in the constitution of the nervous centres in man.]

The nervous centres represent movements, not muscles; chords, not notes. This is evident from the effects of destroying lesions of the corpus striatum. From a small lesion of this body there does not result paralysis of a small part of the arm, nor of any such group of muscles as flexors, or extensors; there results partial paralysis of the whole arm, the most special parts of it suffering most. There is loss of a certain number of movements of the limb. Let us take a more striking example: in cases of very grave lesion of the corpus striatum (that is, of a centre far above the supposed deep origins of the ocular motor-nerves), there is, besides palsy of the face, arm, and leg, an ocular palsy. Now this palsy is not of the sixth nerve, nor of the third nerve, nor of the fourth, nor of any one muscle, nor of any random grouping of muscles. It is a loss of a highly

special and widely associated movement; the patient has lost power to look to that side on which his body is paralysed; there is what is commonly called lateral deviation of the eyes. Similarly, in convulsion there is a development of movements. In a convulsion beginning in the hand, the spasm creeps up the whole limb, developing first the movements of the most special parts of it, but not picking out such groups of muscles as flexors or extensors. Among other movements, there is at a certain stage a development of that of the eyes for "looking" to one side. In this case the two eyes are turned to the side of the body convulsed. We must, however, draw attention to a very important qualification with which the expression "development of movements" is to be used.

Both in Dr. Ferrier's experiments and in cases of convulsion from disease in man, the results of the discharges (since they are sudden, excessive, and very local) are only exhibitions of the movements represented in the parts discharged in the rough. A great number of different movements are developed at once. And it must not be forgotten that not only are the discharges unnatural in being excessive, sudden, and temporary, but also in that they are very local. We are reminded of the effects of putting one muscle into strong action by Faradising it; the result is a mere caricature of a normal movement. Duchenne insists that a muscle is never singly in action in health, except perhaps in the case of the facial muscles.

But in spite of these drawbacks, the study of discharging lesions, whether induced by Faradisation or by disease, is of great value. My own opinion is that there is no other way of finding out what movements parts of the convolutions near to the corpus striatum represent. The reason for thinking so is that the other process of experimentation—that by destruction of small parts of the cerebral hemisphere—produces no obvious symptoms—no obvious loss of movements, or no special loss at any rate. But it would be a great error to infer that the part destroyed did not represent special movements. The bearing of Ferrier's researches is very direct on this matter. For if we discharge that part, destruction of which produces no loss of movement, there will be a presentation of a mass of movements. See, then, the clearing of the paradox. Disease of the convolutions sometimes does, and sometimes does not, produce symptoms. The word disease is used vaguely; so far as it involves destruction there are no symptoms, but there are symptoms from discharge. The speculation I have put forward to explain these paradoxical results is as follows:—

To begin with a "motor" centre. The study of cases of hemiplegia shows the constitution of the corpus striatum to be such, that each part of it represents movements of the whole of

the parts which that organ governs. So to speak, the corpus striatum is a mass of corpora striata, each one of which represents faintly, and each in some slightly different manner, the whole of the parts which the corpus striatum in full represents in greater degree. Now, to pass to the hemisphere. The convolutions in the region of the corpus striatum are the corpus striatum "raised to a higher power." Each part of the brain in this region re-represents the whole of the movements which have been represented in the corpus striatum; so then, if any one part of the brain in this region be destroyed, there is no obvious loss of movements, because the movements it represented are still represented in each neighbouring part, although in different degrees and orders. But for this very reason, if any one part be strongly discharged, vast numbers of movements are developed.

I now mention illustrative cases. Although I can only take enough space to give a mere outline of the cases, I think they show plainly that some of the "experiments of disease" on man are, notwithstanding the special differences I have insisted on, fundamentally like the experiments of physiologists on animals; that a large and definite part of the study of nervous diseases, of convulsion in particular, must be put on an anatomical and physiological basis; and that Ferrier's researches will be a most valuable help in thus methodising our work—in making it less empirical and more scientific.

The first illustration is an outline of a case I have already published. A man had convulsions, each of which began in his left thumb. He died: we found no other disease in his brain than a tubercle, the size of a hazel nut, in the hinder part of the third right frontal convolution. It will be particularly interesting to me to know what effects Dr. Ferrier will obtain by Faradising the homologous part of a monkey's brain, for he tells me that these animals will shortly be subjected to experiment. Theoretically, I should not expect an identical convulsion, but an homologous one. I will, before giving the second illustration, remark on this point, and, at the same time, state some facts which show that the study of cases of convulsion bears on what is conveniently, if not correctly, called the physiology of the mind. One constantly hears, however, that the convolutions are not for movements, but for "ideas," "memory," &c. Yet those who use psychological phraseology to describe symptoms of disease—"loss of memory for words," "chorea the result of disorder of volition," &c.—have as much as other people to seek the anatomical and physiological substrata of mental phenomena.

Among the fits which begin unilaterally, the commonest are those in which the spasm starts in the index finger and thumb.

This is significant. It is an illustration of what I believe to be the law of the effects of lesions of the brain. In evolution (development, education, &c.), the progress is from the general to the special. In the opposite process of dissolution the more special parts suffer first. I generally use the term "voluntary" instead of "special;" it is a convenient counterpart to the term automatic. In physiological language, a voluntary part—the hand, for example—is one which has the greater number of different movements at a greater number of different intervals—shortly, the most varied uses; an automatic part—the chest, for example—is one which has the greater number of similar movements at the greater number of equal intervals—shortly, more similar uses. In brain-diseases parts suffer the more as they are voluntary, and the less as they are automatic. Now the thumb and index finger are the most voluntary or specialised parts of the body; hence the suggestiveness of the case I mention. The thumb in man has a distinct flexor longus pollicis. In the Anthropomorpha, Huxley says: "The flexor pollicis is more or less closely connected with the flexor communis perforans, or with that part of the muscle which goes to the index digit." On the intellectual importance of this muscle Duchenne insists strongly. "En somme, ces faits cliniques demontrent que le long fléchisseur du pouce est l'un des muscles qui sont essentiellement destinés, chez l'homme, aux usages manuels les plus delicats; à tenir et à conduire la plume, le crayon, le pinceau, l'aiguille, &c.; qu'il aide, en un mot, à l'exécution des travaux manuels qui sont à la hauteur de son intelligence supérieure." And in his work on Electrisation, he says that when the small muscles of the thenar eminence are atrophied, the hand loses its distinctive human character and approaches that of the monkey. The thumb in the monkey is less specialised than in man. If, then, we discover in a monkey the homologue of the part discharged in my patient, we shall expect from its discharge a fit of a less special kind; for example, not a fit beginning in its pollex, but more likely one beginning in the whole of its five comparatively little differential digits at once, if not in the whole arm.

I have yet to publish the case of a woman who had fits beginning in her left great toe (where fits beginning in the foot nearly always start); there was a small tumour in her right hemisphere. My colleague, Dr. Gowers, made a careful examination of the brain for me: he found that the tumour involved the lower part of the ascending frontal convolution. Although there were other lesions in this woman's brain—local indurations—there is a strong probability, amounting almost to certainty, that the fits depended on discharge of that part in which lay the tumour. Here, again, I await Dr. Ferrier's

further researches in comparative physiology of the convolutions of the left hemisphere.

In the Medical Mirror of September 1869, I published the case of a man who had fits affecting the right arm. In this case there was a tumour in the hinder part of the first (uppermost) frontal convolution. (There was also a tumour in each lateral lobe of the cerebellum, to which I traced no symptoms.) I did not see this man in a fit; his arm was paralysed after the first seizure.

Lately I was allowed by Mr. Soutter to see a patient of his who had literally innumerable fits limited to the right arm. Mr. Soutter witnessed many; I saw several. The spasm passed down the arm except in the later fits, then it passed up. Shortly before the patient's death she had, Mr. Soutter tells me, universal convulsion. Here I correctly predicted disease of the hinder part of the first (uppermost) frontal convolution —not from physiological knowledge, but because of what I found in the other case just mentioned. Once more I ask, What would be the homologous series of convulsions from the artificial excitation of the homologous parts in a series of animals lower and lower in the scale.

[Dr. Jackson adds in a foot note:]

The significance of the fact, that the hand is the part in which convulsions, beginning unilaterally, most often start; that the arm suffers first, or most, or both, in the greater number of motor affections from brain-disease (hemiplegia, chorea, paralysis agitans), will be better realised after reading Herbert Spencer's remarks on lacteal organs, in chap. viii., vol. i., p. 359, of his Psychology (second edition), from which I give these extracts.

He points out and shows the significance of the "striking instances which the animal kingdom presents of unusual sagacity co-existing with unusual development of organs, which, by the help of complex muscular arrangements, give complex tactual impressions." After remarking, that it will perhaps be difficult to understand why touch, the simplest and earliest sense, should in its higher form be more than any other sense associated with the advance of intelligence, he says: "The explanation lies in the fact that tactual impressions are those into which all other impressions have to be translated before their meanings can be known." Of the human hand:—"All that we need here notice is, the extent to which, in the human race, a perfect tactual apparatus subserves the highest processes of the intellect. I do not mean merely that the tangible attributes of things have been rendered completely cognisable by the complex and versatile

adjustments of the human hands, and that the accompanying manipulative powers have made possible those populous societies in which alone a wide intelligence can be evolved. I mean that the most far-reaching cognitions, and inferences the most remote from perception, have their roots in the definitely combined impressions which the human hands can receive."

The study of cases of disease of the nervous system appears to me to supply continual illustrations of the correctness of many of Spencer's deductions.—*British Medical Journal, May* 10, 1873, *p.* 531.

24.—MIGRAINE FROM INJURY TO THE HEAD.

By Dr. DOUGLAS MORTON, Physician to Louisville Hospital, U.S.

I will offer a number of facts, drawn from my own personal experience of migraine, that throw, I think, a flood of light upon part of the subject out of which arose the discussion between Dr. Anstie and Dr. Dale.

In my judgment these facts very distinctly support the theory of a neurotic origin. The symptoms attending all my attacks, in order and character, have been just the same; and each attack, as far as I have been able to learn, may be properly regarded as a *typical* expression of the affection. But the point to which I would especially call your attention is, that *my first attack was directly and immediately induced by an injury.*

About fourteen years ago, while a student at college, I had a severe fall from a swing in a gymnasium, my head striking with great violence the hard, beaten earth beneath. At the moment I felt no pain, but experienced a horrible sensation, which I suppose is best described by the word *stunned.* I went at once to my room, and when, after a short time, my symptoms had improved a little, I took up a Greek book in which I had to prepare a lesson for that day. To my dismay I found that I could see only the left half of the open book; and if I directed my attention exclusively to a line or a word, only the left half of that. My first impression was that my right eye had been injured; and when I looked in a mirror to find out if this were the case, I was startled at not being able to see any part of the right side of my face. I held my hand over one and then the other of my eyes, and discovered, as I supposed at the moment, that the injury to my eyes consisted in destruction of vision in the right halves of both, for with either closed I could see the left halves of objects I looked at with the other. As yet, as well as I recollect, my headache had not commenced. I returned to my book and tried to translate, but found this impossible—the meanings of the words had escaped my memory almost entirely. I could scarcely pronounce a word—the

difficulty seeming to be that I could not well retain a conception of the sound represented by more than one letter at once. In a short time I began to suffer headache, but at first it was not severe, and I went out and joined a group of students on the college grounds, in whose conversation I ventured to participate. I found, however, that I was the subject of a peculiar kind of aphasia—I could intelligently follow their conversation, but had not the power to select words suited to the expression of what I wished to say. I hit upon words in a most curiously random way—a verb, perhaps, for a noun, or an adverb for a preposition. My headache continued to increase in severity, and in another hour became almost intolerable. Now I began to suffer nausea, and vomited, but the disturbance of vision had entirely passed away. I soon recovered from the sickness, though the headache lasted the remainder of the day—about eight hours.

Had I suffered no subsequent attack of migraine, I should now regard the condition which I have described as simply that to which "concussion of the brain" gives rise—the subjective symptoms of which, by the peculiar circumstances of my case, I could observe closely, and give a better account of than we can usually elicit from patients.

After an interval of about seven years I had my second attack, and on this occasion noticed phenomena that escaped my attention in the preceding one, viz. the appearance of such a figure as I have represented in the enclosed drawing, trembling in space, seemingly a few feet from my eyes, and a sensation of numbness in my right cheek and the right side of my tongue, which came on with the headache (if my memory is correct) after the optical phenomena had disappeared.

Since that time I have had several other attacks, each being expressed by essentially the same set of symptoms. The form even of the phantasm which quivers before my eyes in the outset of every attack has continued almost exactly the same.

On studying the etiology of my case, I have not met with the success Dr. Dale appears to have met with in his. In no instance have I been able to connect an attack with any deviation from what is my usual mode of life, whether as to diet, amount of work, mental or physical, amount of sleep, or exposure to weather. Nothing whatever in my experience has seemed to point to disturbance of digestion as a cause. I have suffered headache from the latter cause, and I suppose from most of the causes that usually occasion headaches in other people; but my *sick*-headache stands alone—*sui generis*—in its peculiar group of symptoms and in the obscurity of its etiology.

In my experiments in treatment I have been more fortunate;

indeed my results have been positive, and, happily, decidedly favourable. I have experienced benefit from taking a cup of strong tea at the occurrence of the first symptoms; so, also, from a tumbler of sherry wine; but *the* remedy *par excellence* I have found in guarana. The specimen of this drug that I got was put up in a package, containing twelve powders, of, I, suppose, about thirty grains each. I took four of those in two hours, and, though perfect relief was not afforded, my suffering was yet diminished to such a degree, that the attack, whose prodromes foreboded unusual severity, proved to be a very small affair indeed. Having tried the guarana only once, it may appear unscientific to speak so enthusiastically of *it* as *the* remedy; but so intense had always been my suffering, and so great was the relief following the medicine, that it is impossible for me to regard their connection as otherwise than a *propter hoc* relation. I have seen one or two other cases reported in which great benefit was derived from guarana. The remarks I have made upon treatment, here, however, are of secondary consideration, my main object being to give the circumstances attending my first attack, as, to my mind, they appear to throw light upon the true character of the affection.—*Practitioner*, *Aug.* 1873, *p.* 100.

25.—ON GUARANA AS A REMEDY FOR SICK HEADACHE.

By Dr. W. MACDOWALL, Assistant Medical Officer and Pathologist, West Riding Asylum.

The cases of sick-headache treated with guarana are as follow:—

Case 1.—M. N., aged 34. This lady has suffered from sick-headache for a great number of years, indeed from childhood. Her attacks were very severe until about eight years ago, when she removed from an inland country town to a residence near the sea. She states that until she was about twenty years of age she was in somewhat delicate health; not suffering from any disease in particular, so far as she knows, excepting her headaches, but was languid, weakly, and unfit for active exertion or occupation. After that date her health improved, and has continued excellent up to the present time. She attributes this establishment of her strength to a prolonged residence in the country.

Since early childhood she has suffered from sick-headache, and her tortures have continued with increased severity since she fell downstairs and injured her head. This accident occurred when she was about nine years of age. She was unconscious for some hours after the fall, and was confined to bed for some days.

The following is her account of the sick-headaches as they occurred before they decreased in severity eight years ago:—They occurred, as a rule, every three weeks; and she never escaped for longer than a month. She experienced no previous derangement of digestion; but, when attack was imminent, she frequently had uneasy sensations at the pit of the stomach. These would gradually increase in intensity during a few hours, and generally ended in vomiting. As a rule, however, these preliminary stomachic symptoms were absent. Her first head-symptom was a slight throbbing pain, passing, as it were, between the temples or between the forehead and occiput. At the same time a feeling of great depression, with sickness, always appeared. The characteristic symptoms then gradually increased in intensity, and in from three to six hours the attack usually reached its height. Before this stage, however, had been attained, she became quite prostrate, and was obliged to go to bed. These attacks were always, sooner or later, accompanied by vomiting; the act of emptying the stomach increased the pain in the head to an almost unendurable degree; but when it was completely emptied of its contents—usually highly bilious matter—some relief to all the symptoms followed. On an average, the headache continued at its maximum during six or seven hours; an hour or two after vomiting the patient generally fell asleep, and then awoke in the morning free from all her previous miseries, but feeling extremely weak and languid.

Shortly, it may be stated that the attacks of sick-headache during the past eight years have been, in their leading features, similar to those already described, but somewhat less intense.

The following have been the results of treatment with guarana:—

Feb. 1. Slight symptoms appeared shortly after rising in the morning, and continued to get worse until 7 p.m. At this time she presented all the indications of an intense attack, and retired to her room, quite unable to bear up longer against her suffering. She now took 3 ss of guarana, with the, to her, very unexpected and satisfactory result, that in about twenty minutes all sickness and headache had disappeared; and she felt, as she expressed it, like another woman. The drug appeared to her to act like a charm; and so much pleased was she with the result, that she presented herself for inspection to some who had but shortly before witnessed her distressed condition. The improvement in her state was most marked and surprising, for, from being almost quite prostrate, she was at once able to resume her somewhat arduous duties. She also escaped without the occurrence of vomiting.

Feb. 22. During the morning, the usual premonitory symptoms appeared. At once she took ʒ ss guarana, the result being, that instead of the attack running through all its stages, it disappeared entirely in less than half an hour.

April 8. She awoke in a state of considerable depression; during the day a typical sick-headache became developed; but from circumstances she did not take any medicine until 5 p.m., at which time the symptoms were very severe. No benefit resulted from the half-drachm of guarana which she then took. At 9 p.m., still feeling extremely ill, she took another ʒ ss, and in an hour all the symptoms had disappeared. During the whole day she had been unable to take any food, yet at 10 p.m, all nausea had so entirely disappeared, that she was able to take a hearty supper.

Up to the present time (May 8) she has had no return of sick headache.

Case 2.—R. D., aged 30; nurse. She states that she had suffered from sick-headache from childhood. The attacks, however, have not been quite so severe during the past four years. This change in their character she associates with her change of residence, for four years ago she left her native county, Leicester, and has since that date resided in Yorkshire. During childhood the attacks were not frequent—perhaps every two or three months; but after reaching puberty, they occurred regularly every month for a number of years. They invariably preceded the appearance of the menstrual flow. As she advanced in age, the attacks occurred at other times besides the menstrual one, so that on an average they came on once a fortnight. None of her brothers or sisters were affected like herself, but her mother suffered severely from sick-headache as long as she lived.

The usual course of attack is as follows:—After slight headache has existed about half an hour, she generally becomes slightly sick. The pain is always confined to the forehead, and is described by her as being of a burning, throbbing character. As the attack proceeds, the pain in the head becomes almost intolerable, and she is in the habit of going to bed, that she may obtain some slight relief through rest and quiet. The sickness rarely terminates in vomiting, but there is intense loathing of food, her appetite having entirely disappeared. On rare occasions the headache has remained at its maximum of severity for from twelve to twenty-four hours; but the average duration of this period may be stated at about six or eight hours. When an attack is disappearing, the sickness always disappears first; the pain in the head then lessens, becoming of a dull and depressing character; then, in about two hours, all disagreeable symptoms disappear, and the

patient regains her usual health, being quite free from any depression or exhaustion.

Since her residence in Yorkshire, her headaches have not, as a rule, been so frequent, occurring not oftener than every six weeks; and it has even happened that she has been free from them for three months.

On four occasions she has been treated with guarana, and always with the happiest results.

March 1. She awoke in the morning suffering from severe sick-headache, although at that time it had not obtained the usual maximum of intensity. She took half a drachm of guarana at once, and experienced decided relief in about half an hour. During the remainder of the day she suffered from a dull uneasiness in the head, bnt was quite able to go about her usual duties.

March 31. After travelling, the premonitory symptoms of an attack appeared. On reaching home she at once took half a drachm of guarana, and in less than an hour was quite free from all inconvenience.

April 27. Awoke with headache, but being inclined to try if it would pass off without treatment, she delayed taking her usual dose of guarana. Feeling no better, however, she lay down in bed about 2 p.m. for a short time, but without relief. She then took half a drachm, relieving the sickness immediately, and the headache very soon afterwards.

May 10. About midnight she began to suffer severely from sick-headache, and continued ill all night. In the morning she took a little tea, which made her sick, but with no relief to headache. At 8 a.m. she took the usual dose of guarana, but with no result; at 10.30 a.m. it was repeated with like result; at 2 p.m. another half-drachm was taken, which gave relief very soon afterwards.

Case 3.—M. M., aged 25, single, has suffered from sick-headaches from childhood; indeed, she does not remember a period of her life when she did not suffer from them. When she was a child, they occurred about once a fortnight; but when she reached puberty, they became less frequent, the intervals increasing to four or five weeks. There has never been any connection between the occurrence of menstruation and the date of appearance of a sick-headache. For a number of years this young woman has been in delicate health. She labours under phthisis of a very chronic description, though the disease has occasionally assumed a more active form for brief periods, and there have been several rather smart attacks of hæmoptysis. Of late, however, with proper attention to her health, she has continued in moderately fair

general condition, and quite able for her work as a nurse, but at the same time very much troubled with headaches.

Since her general health became delicate, her attacks of sick-headache have been much more frequent, and indiscretion in diet brings on one at once. As a rule they begin during the night. The first symptom is a dull throbbing pain in the temples; it gradually increases; then sickness comes on; so that in a few hours she is frequently necessitated to lie down in bed. Should the attack prove a mild one, it may pass off in six or seven hours; but it is more common for it to continue during a whole day. On rare occasions, when particularly severe, it has continued for two days. After a sick-headache has continued at its maximum for about an hour, the patient invariably vomits a small quantity of bilious fluid, sometimes with some slight relief to the pain in the head.

April 16. Patient awoke in the morning with sick-headache, which continued intense during the day, until 6.30 p.m., when she took half a drachm of guarana, and in half an hour she felt quite relieved.

May 10. Awoke this morning: sick-headache in first stage. Shortly after taking the usual dose of guarana, the symptoms began to abate; and by 9 a.m. she was quite free from all discomfort.

[Several other cases are related of the same description.]

—*Practitioner, Sept.* 1873, *p.* 161.

26.—ON THE THERAPEUTICAL USE OF ELECTRICITY.

By the EDITOR OF THE MEDICAL TIMES AND GAZETTE.

[The profession in general does not value this remedy as it ought to be valued. In fact, it is not studied or understood sufficiently. Although it cannot renew nervous tissue, and cannot cure some of the worst cases of paralysis, it can do a good deal if the cases be properly selected. It can "soothe or stimulate diseased muscle and nerve, cure or relieve over-action (spasm) and pain (neuralgia), some forms of aphonia and of periphral paralysis, renew the nutrition of muscles wasting from disease, restore the circulation, and raise the temperature of a paralysed limb, and recall or lower sensation."]

In relieving or curing pain, its therapeutic value is invaluable, and has been established beyond the possibility of dispute or doubt. In all the true neuralgiæ, and especially perhaps in the very worst of them—the most terrible and savagely intense of all painful affections—electricity rightly

chosen and applied, displays often a remedial power not only unsurpassed, but unrivalled by any other known means. But this use of it is not so generally known or understood even yet (among us, at least) as is its employment in the different forms of paralysis. And the reason of this is perhaps not far to seek. Duchenne was the apostle of faradisation, Remak of the constant current; and the teaching of Duchenne has always been more accessible to, and better known by, the mass of the profession than has been the teaching of the German school. Moreover, Faraday was the discoverer of the induced electric current, and that fact has also not a little helped to make faradisation more popular and better known with us than the constant current; consequently, it came about that for a long time it was generally believed in this country that the rotatory electro-magnetic, or magneto-electric machines, were sufficient for all electro-therapeutic purposes; and, indeed, very many of our brethren still suppose that it is the case, and when they have obtained one of these machines believe that they are fully equipped for the remedial employment of "electricity." But it is absolutely certain that in the treatment or painful affections the constant current has a far wider usefulness and a far higher value than faradisation has; and we suspect that even the strongest believers in faradisation will allow, with Dr. Beard, of New York, that in the treatment of true neuralgia "the galvanic (*i.e.*, the voltaic or constant) current will oftentimes succeed where the best faradic fails, and even in those cases where faradisation relieves, and promises to cure, galvanism may do the work in a much shorter time."

In some painful affections, and some that are often improperly called neuralgiæ, faradisation is undoubtedly of great use; but in the neuralgiæ proper, the constant voltaic current is almost always the form of electricity which ought to be employed, both the electrodes being firmly pressed upon the skin and held there, neither of them being moved about: that is, the current must be the voltaic, and must be *constant*, not interrupted; and the electrodes must be applied so as to include in the circuit the affected nerve. As to the number of the cells employed, this must be regulated by the sensations of the patient, and according to the region affected; they should be as many as can be borne without pain or serious discomfort; and in the face five cells will be fully enough to begin with. In this part fifteen or twenty cells will most likely be the highest power that can be borne or safely employed, the application being discontinued for a while and the number of cells reduced whenever any giddiness occurs. Elsewhere a higher power may be used. Whether the doctrine that in true neuralgia the posterior root of the nerve is the seat of disease be

accepted or not, it is, we suppose, generally held that it is best to place one of the electrodes as nearly as may be over the point of origin of the affected nerves, and the other over the painful spot; or, should there be more than one painful spot, then over the several spots in succession. The direction of the current has by some been considered of importance, and directions have been given to apply always the positive electrode to the seat of disease; but the necessity for this is not clear; and some experts are convinced that the direction of the current—whether it should be ascending or descending—is not therapeutically a matter of any importance whatever. The electrodes should be furnished with well-wetted sponges of large size, and the application be made for short periods—about five minutes—and repeated oftener than once a day if necessary.

Cases of neuralgia of the most intense character have been not only rapidly relieved, but permanently cured by this means. Niemeyer's two cases are perhaps the most striking. Dr. Anstie, in quoting them, observes—"The patients were respectively aged 64 and 74, and the duration of the neuralgia had been respectively five and twenty-nine years; in both the pain was of the most severe type, and in both the success was most striking. In one, every possible variety of medication and several distinct surgical operations for excision of portions of the affected nerves had been quite vainly tried." Such brilliant success as this is not always to be expected; but many very striking cases besides have been recorded. Dr. Russell Reynolds has mentioned the case of a lady who for twenty years had suffered from very severe neuralgia of the ophthalmic branch of the fifth nerve, recurring every day, and greatly injuring her general health, and which was not merely relieved, but removed, by a single application of the constant current. Dr. Althaus, also, has published the case of a lady who for twenty-five years had suffered from the most violent neuralgia on the left side of the face. Three applications of the constant current from four cells of Bunsen's battery were, he says, "sufficient to cure her of a disease which for twenty-five years had embittered her life."

While all the best authorities on the use of the constant current agree in estimating very highly its remedial power in neuralgic affections, they vary in their experience of its value in the different forms of this disease. Thus, Eulenberg, whose experience has been very large, thinks very highly of it as a palliative in trigeminal neuralgia, but rather doubtfully as a curative; while Dr. Anstie credits it with more of a curative power. And while Eulenberg has found sciatica the most curable of the neuralgiæ by this means, Dr. Beard has had least success as to *cure* in this form of neuralgia, though he has

generally obtained *relief*. His experience leads him to consider gastralgia as more amenable to the constant current than the other remedies.

In migraine, or true sick headache, the constant current has been found very beneficial, the electrodes being applied to each temple or to the mastoid processes. A very low power must be used, two or three cells. The application must not be for longer than about sixty seconds, and it must be discontinued on the occurrence of any giddiness, or if the patient exhibits any symptom of uneasiness in the head or the epigastrium. In all the true neuralgiæ, then, the diagnosis having once been clearly made, the constant current is the form of electricity to use; and used rightly, so as to act gently and steadily, without exciting pain or other nervous disturbance, it may be employed fearlessly and with immense advantage.

This kind of electricity has also been successfully employed in toothache. M. Bouchaud states (*vide Bulletin General de Therapeutique*, July 15)—"If in odontalgia we employ a continuous or constant current, derived from a mean of ten elements, applying the positive pole to the cheek on a level with the painful tooth, and the negative pole to the anterior lateral region of the neck, we are enabled almost always in the course of a few minutes to procure nearly absolute relief, which in the majority of cases persists indefinitely." M. Bouchaud's paper on the subject is well illustrated by cases; and he has found the constant current useful even when the teeth were carious and the gums inflamed and swollen.

If for any reason it is determined to apply the faradic current in neuralgia—and sometimes it is beneficial—the rough coarse current supplied by a rotatory magneto-electric machine should not be ventured on, but the most gentle and steady current of this kind derivable from a volta-faradic apparatus. In some painful affections, however, faradisation may be employed, not only usefully, but with much greater advantage than the constant current. Such are the pseudo-neuralgiæ called "hysterical," and the hysterical hyperæsthesiæ. In these affections it is desirable or necessary to excite pain, and the ordinary rotatory machines will serve for the purpose. The current must be used to affect the skin only, not to penetrate deeper; the skin must therefore be carefully *dried*, and dry metal electrodes used—one being placed on an "indifferent spot," and the other moved about over the affected part: or, the patient holding one of the brass handles of the machine, a coarse wire brush may be applied over the affected region.

The widest field for the application of electricity, and especially for the "localised employment" of it, is no doubt in the

treatment of paralytic conditions; and we will now treat of its application in these various affections.

In paralysis from cerebral disease, great care must be taken not to employ it too soon. If the paralysis has come on suddenly, or if, though it has come on gradually, there is giddiness or pain in the head, or any sensation of weight or fulness of the head, too much caution cannot be used—some fresh mischief may occur under such circumstances, and its occurrence will be attributed to the electrical treatment, though it may be merely and entirely a coincidence. It is far better, in such cases, to wait for a time—for four or more months—before resorting to the employment of electricity, as it is advisable to wait before using any other kind of treatment that would disturb or excite a patient. If none of these head symptoms are present, and the disease has only gradually come on, no such degree of caution is necessary, though then also care should be taken, as we have before said, not to alarm or agitate a patient by a parade of the electrical apparatus or by the mode of using it.

When it is decided to employ electricity, the first step to be taken is to test the excitability of the muscles by the faradic and interrupted-voltaic currents; and the mode in which this is to be done we have already described. The manner in which the muscles respond to the test—that is, the amount of contractility they show—will tell what good may be looked for from the employment of electricity. If the contractility is normal, though voluntary power is abolished, electricity will do nothing towards restoring the power of the will over them; if the contractility is somewhat, but only slightly, diminished, the condition of the muscles may be improved; and when the contractility is found to be markedly reduced, but not abolished, great good may be achieved—the contractility may be gradually brought back till it equals that of the healthy muscles, and then it may be that the paralysis will be found to have been lessened, and now and then even cured. And in all cases this good at least will most likely have been obtained: the nutrition of the muscles will have improved, and colour and warmth have been restored to the skin. This is by itself a gain of great importance, for the muscles and the neighbouring tissues are thus kept in a proper state of fitness and readiness in case the disorder of the brain or of nerve-power that caused the paralysis be restored, and the influence of the will over the muscular part of the locomotive apparatus be regained. And further, it appears to be certain that electrical action on the muscles, peripheral electrical excitement, as it may do harm to, so also may aid the recovery of, the nerve-centres. That it does act on them is shown by the fact that

peripheral galvanisation of the limbs is often observed to excite a peculiar sensation of taste.

Having tested the excitability of the muscles, it will be best, as perhaps the most useful general rule, to treat them by means of the current to which they most readily answer; or to employ the interrupted voltaic current where the excitability is normal, and the faradic where it is diminished.

If voltaism is employed it must be by the interrupted, not the continuous current. Large-sized sponges should be used, well wetted with salt and water, and one (the upper one of the two—that is, the nearest to the central organs of the nervous system) being kept steadily on one spot, the other should be moved slowly down along the course of the muscles; so that every part of each of them is successively brought into the circuit of the current, and by the movement of the lower sponge the current is *interrupted*. And here we may again remark that, therapeutically, it does not matter whether the current is direct or inverse; only it must be remembered that the direct current acts more strongly on the muscles than does the inverse, and that the current must not be so strong as to excite pain: it must be just strong enough to cause contraction of the muscles, not strong enough to hurt.

This rule applies also to the employment of faradisation. The patient is not to be hurt or wearied; and whenever, during the use of any form of electricity, the patient feels faint, or giddy, or sick, or has pain in the head or a sensation of sinking or uneasiness in the epigastric region, the application must be stopped for a while.

If the faradic current is the form of electricity selected, sponges or metallic buttons covered with chamois leather are to be used; and they must be *well* wetted, or the current may only irritate the skin, instead of penetrating to the muscles. Moreover, the rheophores should never be widely separated; it is important to localise the action of the current as strictly as possible; and, with this view, it is best to use poles of such shape and size that both can be easily held in one hand. Then the muscles are to be faradised *seriatim*, each being picked out, and, according to its size, the current being passed through the whole at once, or through different parts successively, so that no portion of it shall escape being electrised. Particular points will be found in the arm and leg (and in other parts, as in the face), at which, on application of the current, the effect on the muscles will be stronger than when it is applied elsewhere. These, called by Duchenne "points of election," are the places at which the motor nerves entering the muscles lie most superficially.

Electrisation may be successfully employed to relieve, or

even cure, the "late rigidity" of hemiplegia. In an early stage of it the tendency to cramp may be removed, and the rigidity cured; and even in old and severe cases, when the nails have dug into the palm of the hand, considerable alleviation and benefit may be obtained. Either the faradic or the interrupted voltaic current, applied to the antagonising muscles, will do good, but the greatest benefit will be gained by applying faradisation to the antagonistic muscles, and aiding it by sending a continuous voltaic current down the rigid muscles; voltaise the flexor muscles, and faradise the extensors.

In *paraplegia*, from myelitis, meningitis, or hemorrhage into the cord, electrisation must not, of course, be used while any active mischief exists; and there will be often much difficulty in determining when it may be safely employed. But the affected muscles may be now and then tested by the interrupted voltaic and the faradic currents, and when it appears that the time has arrived when electrisation may be used, one of the forms, or perhaps both, may be applied in the manner already described. In some of these cases, the only good that can be gained is an improvement in the power of the sphincter of the bladder and rectum: if their power for retention has been lost, electricity will sometimes restore it. To act on the sphincter of the rectum, one sponge-pole, well wetted, of the faradic current, is to be placed on the sacrum, and the other applied to the anus; if the vesical sphincter is to be treated, apply one sponge to the perineum just behind the scrotum, and the other above the symphisis pubis. This will in some cases, to some extent at least, prevent involuntary evacuations, and thus give considerable relief. Or the paralysis may be incomplete, and then larger benefit may be obtained. The good to be looked for may be measured by the state of electro-excitability of the muscles; if entirely abolished, no good can be done, and none if it be normal; but where it is diminished more or less, the nutritive condition at least of the muscles may be improved.

In the "essential paralysis" of infancy and childhood electrisation may be of considerable use if employed before the muscular tissue is too much degenerated. In this form of infantile paralysis the affection comes on suddenly, the faradocontractility of the muscles is more or less diminished, they waste, and their tissue undergoes the fatty degeneration; and they are affected by secondary contractions. Not rarely it will be found that when the affected muscles answer but little, or not at all, to the faradic current, they will respond to a slowly interrupted voltaic current, and are even more than normally sensitive to it. The good that electrisation can do here is to improve and so preserve the nutrition of the muscles, and so

prevent wasting, degeneration, and contraction; and in some cases even more than this may be done, for if the affection of the cord can be cured, the peripheral electrical treatment may forward the cure. Where there is still response to faradism this form of electricity is the best to use, care being of course taken not to frighten or pain the child. Where the irritability to the interrupted voltaic current is *increased*, it will be best to treat the case at first with this current. In a little time it will be found that the reaction to the faradic current is restored, and that the excitability to the interrupted voltaic current lessens; then faradisation only may be employed. The disease is almost invariably a tedious one, and the treatment must be continued for a very long time—for months, or years even. But so long as there is any response to electrisation there is a hope of benefit. Dr. Radcliffe says that "if the paralysed muscles retain their electro-contractility and sensibility, and so show that they have not passed into that state of fatty degeneration into which they always tend to pass eventually, there appears to be scarcely any limit to the time in which improvement, and even complete recovery, is possible."

In *traumatic* paralysis faradisation is invaluable. Brown-Séquard has proved that even the spinal cord may be perfectly restored after traumatic lesion, and it has long been known that nerves will perfectly reunite after division, and when even considerable portions of them have been excised. But after reunion of a nerve has taken place, the muscles supplied by it will often remain paralysed, and continue to waste, and contract unless faradisation be resorted to. Its value in this form of paralysis was largely tried and well proved in America in cases of men wounded in the Civil War; and in a report on "Gunshot Wounds and other Injuries of Nerves," the surgeons in charge say:—"The only important means in the treatment of paralysis from default of innervation is faradisation by the method of Dr. Duchenne. Most of our cases were from wounds that were of old standing when they came under our care, and localised faradisation proved of the utmost value. In some cases at a single application it restored the power of movement to parts long deprived of it, and we have seen very few instances in which there has not been improved nutrition and greater sensibility and strength when we were able to continue it sufficiently long." The faradisation will, of course, be of no use till after the injured nerve has regained its power of conduction; but in recent cases of injury the interrupted voltaic current should be localised in the affected muscles, and direct faradisation be employed when farado-contractility begins to return.

In cases of *local* paralysis, dependent on cold or on metallic poisoning, as from lead or mercury, or on diphtheria, the

employment of electrisation should not be neglected when the affection has resisted other means of treatment. In these cases the faradic and the interrupted voltaic currents, used alternately and localised in the affected muscles, will generally effect a cure. As a rule the farado-contractility is unaltered in muscles paralysed by exposure to cold; but this is not the case in facial paralysis from cold. Here the farado-contractility is diminished or quite lost, while the excitability to the interrupted voltaic current is increased; and faradisation may be employed for weeks without effecting any improvement. In this affection, therefore, the interrupted voltaic current must be used and localised in the muscles till the farado-contractility is restored, and then the two forms may be employed alternately; and in this way cases of even years' standing may be cured or greatly benefited. It will be remembered that in speaking of the diagnostic uses of electricity we mentioned it might be employed to distinguish *lead-palsy* from paralysis caused by cold, for in lead-palsy farado-contractility and sensibility are diminished or lost, while they are normal in paralysis from cold. In lead-palsy the interrupted voltaic current should be applied to the affected muscles. As in other forms of paralysis, where this current acts and the faradic current elicits no response, the sensitiveness to the former is abnormally great, and it is found to lessen as the farado-contractility returns, till each becomes normal. The voltaic current employed must be a slowly interrupted current, and may be gradually abandoned for the faradic current as farado-contractility is restored; and whichever of the two currents is used, it must be localised in the muscles—not indirectly applied.—*Medical Times and Gazette, Sept.* 20, 27, *and Oct.* 18, 1873, *pp.* 327, 356, 436.

27.—ON THE RELATIONS OF FARADIC ELECTRICITY TO PAIN.

By Dr. FRANCIS E. ANSTIE, Editor of the Practitioner.

One of the most important uses of electricity is certainly the relief of pain. And as pain is so common, and often so troublesome a thing to deal with, it is natural that practitioners should eagerly seize hold of what promises to be a new mode of relieving it, without inquiring very minutely as to the precise conditions under which success is most likely to be obtained. The outcome of this indiscriminate employment of the remedy can only be vexatious disappointment in a number of cases, and, in some, a positive aggravation of the sufferings of the patients.

It is time for everyone to understand that in regard to the

treatment of painful affections, faradisation has very distinct and rather narrow boundaries of usefulness; and these it is now our business to define.

1. Faradisation is, in the first place, pre-eminently useful as a *mental counter-irritant* (if we may use the phrase), and therefore will often cure pains of the class vaguely called "hysterical." In the relaxed condition of the nervous tone which belongs to this state, the mere accidental concentration of attention on a particular part of the body will often convert some slightly unusual sensations into the mental perception of actual pain; but this perception is factitious, and can be speedily destroyed by a suitable diversion. For this purpose smart and painful faradisation of the skin is well adapted, and it should be applied to a somewhat larger area than that which the hysterical pain occupies. The surface being carefully dried (and, if necessary, powdered), in order to limit the effects as much as possible to the skin, the current is applied with dry metal or carbon electrodes, one pole being placed on an indifferent spot, the other moved slowly about over the area which we mean to operate upon. The result is, that the mental attention is so scattered and divided among a number of new and sharp impressions, that its morbid concentration is lost, and the sensation of pain disappears. It is very necessary, in the early stages of such cases, to have the electrical apparatus at hand, so that any relapse into the state of actual pain may be promptly dealt with.

2. The pains we have just been speaking of are mainly of a pseudo-neuralgic character; that is to say, they run so far in the direction (or the neighbourhood) of recognisable nerves as to simulate neuralgia pretty closely whilst actually present. Those which are next to be mentioned are not distributed in neuralgic areas, nor do they take the form of spontaneous pain, but that of unnaturally heightened sensitiveness to pressure—the so-called "hysteric hyperæsthesia." This phenomenon, as is well known, is quite superficial, deep and firm pressure giving less pain than that which merely compresses the skin or mucous membrane (for either may be affected). Here, again, faradisation has a most legitimate office; but it needs to be applied with a firm and unsparing hand. Supposing, for instance, that the hyperæsthetic area is on the skin, one uses dry conductors, one being placed five or six inches distant, on an indifferent spot; the other (which should be in the form of the "wire brush") should be so used as to apply a kind of electrical flagellation to the whole of the affected area. This must be kept up for several minutes, with a powerful current; and if the painfulness of the proceeding becomes intolerable, it is better to give the patient a little chloroform and go on (as

Althaus, I believe, first suggested) than to do the thing by halves. It is not often that this procedure fails to cure in one, or any rate two, sittings: most frequently it is at once successful if properly carried out. If the tender surface be mucous—in mouth, rectum, or vagina—the process is still more disagreeable. We use metal conductors as before, but of course cannot employ the wire brush to the mucous surface, but a solid disc or button instead, moving that electrode about over the whole sensitive area.

3. We come now to the large class of cases in which the pains are evidently situated either in the muscular or the tendinous structures, and are closely connected with the degree in which the muscle has been over-exerted in proportion to its nutrition. These "myalgic" affections are also frequently in part immediately excited by the action of external cold and damp upon a similarly under-nourished muscle. They are distinguished by the violent exaggeration of the pain which is caused by any movement of the affected part. There can be no doubt that local faradisation will very usually remove these pains; the only objection to its use is that for the most part the affection is curable by less troublesome and disagreeable means. The hypodermic injection of morphia, in dose of $\frac{1}{6}$ grain or $\frac{1}{4}$ grain, locally, or the muriate of ammonia (internally) in 30-grain doses, will often give prompt relief; or Dr. Reynolds' plan, of sewing a piece of oilskin outside the jersey over the painful part, will do the rest. If faradisation is to be used, however, it is important to remember that it can only be successfully employed in one way; namely, by strictly limiting its action to the skin. For this purpose we must have the skin very dry, and use dry conductors. If we allow the current to penetrate to the muscle and set up contractions, we shall only add to the agony and render the malady more tedious than it is apt to be naturally.

This brings us to the consideration of a particular class of muscular pains, viz., those caused by sudden laceration of fibre, an accident which is not very uncommon. Fresh interest in the faradic treatment of these affections has been raised by a recent case under Professor Broca, of Paris, although the method is already some years old. The patient got the injury in some playful scuffle, the arm being pushed forcibly (in a direction which is not very clearly stated), while the biceps was contracted: the latter muscle gave way. Local faradisation was immediately employed, and though painful it immediately restored the power of raising the hand to the head. The reporter (M. J. Lucas-Championnière) remarks on the strangeness of several circumstances connected with these ruptures of muscular fibres. He does not seem inclined to think that the

contraction of the muscle itself could produce the accident, but rather (with M. Broca) that the already contracted muscle is torn by some violent wrench in the direction of extension; and is especially liable to this if its tissues be in an unhealthy state. But how comes it, he asks, that a method (faradisation) which causes energetic and painful movements of the torn muscle can at once replace it in a condition to perform the usual voluntary movements without pain? We doubt, however, if this be quite an accurate statement of what takes place. In the first place, we do not think that faradisation *which makes the muscle contract* is the real agent at work. In a case where we personally used faradisation with complete success, the patient had torn a portion of the biceps in holding on to a rope which had a heavy weight at the other end; the machine employed was the common rotatory electro-magnetic apparatus, and one of the ordinary brass handles was grasped by the sufferer, while the other was moved about on the skin over the injured muscle without exciting any notable contraction: yet in a few minutes the arm could be flexed and extended painlessly. The rupture was distinct and visible to the eye. There is also, of course, a probability that muscular fibres which are completely divided, even if their elements be made to contract, would give far less pain than if their attachments had remained entire.

4. There is a somewhat indefinite class of so-called rheumatic pains which appear to be seated in the ligaments of joints, or in the periosteum of bones, occasionally also in fasciæ. Of course (and more especially under the periosteal group) there is a great danger of reckoning cases that are really syphilitic; but after careful exclusion of these, a considerable number remain which it is permissible to call chronic fibrous rheumatism. It is very doubtful whether faradisation exercises any direct influence on this affection; for our own part we have not been able to obtain any decidedly positive results, and are inclined to suppose that cases which have appeared to be relieved were really of a myalgic nature. One of the fairest instances in which to test the power of faradisation would be that peculiarly chronic and intractable affection—rheumatism of the plantar fascia: but it must be remembered that, in many persons, the skin of the sole is so dense and resistent, that there will be no chance of applying the electricity with sufficient penetrating force unless the integument has first been thoroughly macerated with warm water.

5. The great question, however, is whether faradisation is of use in true neuralgia; and to this we believe that a decided answer in the negative must be given. It is easy enough, no doubt, to produce evidence of cures by faradisation in what has been called neuralgia by some persons; but it must be remem-

bered that the present tendency of medical classification is to separate true neuralgia very distinctly from the various other pains which have been confounded with it. No pain ought to be called neuralgia unless it obviously follows, on the whole, the track of a recognisable nerve; the affections of this class have a very definite clinical and family history which enables us to identify the disease as clearly as we can identify any malady that exists.

There is ample scope for testing the power of faradisation to relieve the pain of genuine neuralgias, since these are common enough and, in several forms, are so typical that they cannot be mistaken. Personally we have tried faradisation over and over again in distinct trigeminal, brachial, sciatic, and cervico-occipital neuralgias, and the general result of our experience is quite clear. In a considerable number of cases, probably the majority, the process only aggravates the pain; and we have known instances in which a neuralgia which, to all appearances, had begun to recede, was at once wakened up and made worse than ever, and more rebellious to the influence of ordinary palliatives than before. In other cases we have seen momentary apparent benefit, which seemed due to the nervous surprise, so to speak; but such benefit does not endure (as it often does in hysterical pseudo-neuralgia), the pain soon returns again, and, if anything, with increased violence. In other cases, again, faradisation has produced no effect whatever, good or bad.

On inquiring further into the matter there are at least two reasons why faradisation would be likely to prove either indifferent or hurtful in the treatment of true neuralgia. If the current does not penetrate beneath the skin, the only effect produced will be a superficial irritation which might (in a reflex manner) possibly act beneficially if it could be kept up for a long time (as the effect of a blister is), which however cannot be done. On the other hand, supposing the current to penetrate more deeply, it is likely to do mischief in either or both of the following ways:—If it encounters the nerve, it inflicts upon it the jarring sensation which the very nature of the interrupted current necessarily involves, and which we cannot but suppose must be exceedingly inimical to the restoration of the irregular nerve action to a state of equilibrium. On the other hand, it is difficult or almost impossible to prevent the *penetrating* current from exciting the surrounding muscles to contraction: and it is well known that the most certain aggravation of neuralgia is produced by the dragging and squeezing of muscular movement: the strongest examples being seen in the effects of mastication in neuralgia of the third division of the fifth, and of certain complicated muscular actions (*e.g.* pianoforte playing) in brachial neuralgia.

The above remarks give a rough outline of the reasons why faradisation is generally useless or objectionable in true neuralgia. In that disease it is at once desirable to act on the nerve itself, and yet to act with the greatest gentleness and steadiness. The true neuralgias, as we have tried to show elsewhere, involve a real change (it matters little now whether we call it atrophy or rhythmical molecular alteration) in the posterior or sensory root of the nerve, and this state is best combated by such means as tend gradually and steadily to restore the nutrition of the nerve. And, on the other hand, clinical experience teaches us, very positively, that there are no such terrible excitors and aggravators of neuralgic pains as repeated intermittent impulses. The pulsation of a vessel, for example, which presses upon a neuralgic nerve, sends thrill after thrill of pain through it, till at last the torture becomes unbearable, even to a patient who perhaps could endure well enough a constant though severe suffering. Now, the intermittency of the interrupted current is a matter of degree, and consequently the amount of mischief that may be done by faradisation varies very much according to the apparatus used and the exact mode of using it. This brings us naturally to the final remarks which we have to make, which concern the question of apparatus.

A fairly good faradiser for those cases in which we desire to inflict a good deal of pain (or at least a considerable nervous perturbation), is the ordinary rotatory magneto-electric machine, which, till very lately, has been absurdly supposed, in this country, to be efficient for all electro-therapeutic purposes whatever. There is no question, however, that it does very well for hysterical pseudo-neuralgia and hysterical hyperæsthesia, especially if the patient be made to hold one of the brass handles, and a very coarse wire brush be used as the other electrode, and freely applied to the affected part. But it is usually (except for the mere saving of trouble) better to use a volta-faradaic apparatus—*i.e.*, one in which the motor power is got from a galvanic or voltaic cell; and for the purpose in hand a very simple and inexpensive apparatus will do, such as Hearder's "20-power" machine, though of course it is better, if possible, to supply oneself with a thoroughly complete instrument, like those of Stöhrer or of Messrs. Weiss.

In dealing, however, with painful affections by means of faradisation, there are more considerations than one to be taken into account in the choice of the apparatus to be used. Besides mere questions of convenience in application, there is the question, what degree of frequency and regularity do we desire in the interruptions of the current?

As a general rule, it is true that, "in irritation by induced

currents, the more rapid the interruptions the greater is the pain produced." The interruptions are rendered more frequent, or lower, by manipulations with screws, which place the interrupting hammer nearer to or farther from the platinum point. But in dealing with actual clinical cases, it will not be found that the therapeutic facts exactly correspond with this rule. The object being to create intense nervous surprise and distraction, it will not unfrequently be found that, a current of sufficient intensity having been provided, a series of interruptions which is slow on the whole, but has an *irregular rhythm*, is more effective than a mechanically regular series of rapid interruptions. It is not easy to lay down precise rules here, but the following is about accurate as a representation of my own experience:—In proportion as the malady is beyond the patient's own control, it is desirable to employ a regular series of intermissions, and *vice versâ*. Given, that is to say, a patient with none of the common convulsive phenomena of hysteria, but with some definite paralysis or anæsthesia, and whose skin presents a patch of acute hysteric tenderness; then I advise the use of a tolerably intense current with rapid and even intermissions. The patient should be placed under chloroform, and the dried skin should be well fustigated with the wire brush of a Stöhrer's or Weiss's faradiser, the secondary current being employed, of high strength. But if the affection be of that exceedingly common kind in which the morbidness of the will has much to do with the painful sensations themselves, then it is a good plan to proceed differently. For such patients no chloroform should be allowed; the electrodes should be brass discs, and a common magneto-electric apparatus may be employed. The current being arranged at a high degree of intensity, the driving-wheel is to be urged at a varying and irregular pace: it will be seldom that the morbid sensations do not give way after a few sittings conducted in this manner.

In speaking of the limitations of faradisation as a remedy for pain, we have not forgotten, though we have left so far unmentioned, the methods which are called electric pencil and electric moxa, and which aim at producing a more severe and continuous painful impression. Our own experience is decidedly in favour of the abolition of such methods of treating pain; they ought only to be used, we think, for cases in which there is every reason to think that the whole symptoms are mere malingering. If it be really desirable to push the skin irritation beyond the limits already traced, it will be much better to lay aside electricity and resort to the more prolonged and gradual excitement which can be produced by blisters; and if these fail (especially when used in conjunction with hypodermic injections of morphia), we must resort to the con-

stant current. In all the true neuralgias there cannot now be a question that the latter is essentially the right, and the only right, form of electricity to be employed.

As a parting word of insistance upon the necessity for not employing faradisation except within its proper limits, we must especially dwell upon one variety of malpractice which has not unfrequently been followed by very disastrous results. We have had occasion to know that in cases of rachialgia (spine-ache) it is not uncommon for practitioners to apply one pole of a common magneto-electric apparatus to the nape of the neck and the other to the sacrum or to the feet (in a water bath), turning on a pretty sharp current. Such practice is not merely senseless, it may prove very dangerous; for the shock which is thus given to the system is much more serious than that produced by any amount of pain inflicted upon a limited skin area. There is no reason to think that such faradism has any direct effect upon the spinal cord, but in a reflex manner it may have a most serious effect; and the greatest general prostration, or even paraplegia, may be produced. It is really a heavy disgrace to the profession that such utterly reckless proceedings can possibly occur as some which we have personally known to take place in the supposed electrical treatment of what the practitioner was pleased to call hysterical pains.—*Practitioner, June* 1873, *p.* 351.

28.—TREATMENT OF NEURALGIA BY ELECTRICITY.

By Dr. GEORGE M. BEARD, New York.

[Electro-therapeutics are exciting a good deal of attention just now, and probably no subject has been more neglected by medical men.

In order to do good by this remedy, you must not confound neuralgia with rheumatism, nor with hysteria.]

The generally accepted notion that the disease is where the pain is, has led physicians to apply the current solely to the seat of the pain. Logic and experience show that the seat of the disease, the central nervous system, should also be treated.

Then, again, it has not been recognised that the different forms of neuralgia are but manifestations of a constitutional tendency, and should be treated accordingly.

The whole central nervous system is invaded by a mysterious foe that darts out along the great pathways of nerve-force in any direction and where least expected. The special manifestations of the neuralgic tendency are often different at different periods of life.

I have now under my observation a gentleman of middle life, who, in youth, suffered from facial neuralgia; in later years, chronic cervico-brachial neuralgia has perpetually harassed him; during the past year he was prostrated by a terrible attack of gastralgia, and just now he is recovering from an invasion of sciatica. To treat such cases by merely local electrisation, whether it be central or peripheral, or both combined, may indeed palliate or relieve; but to expect permanent victory over the neuralgic tendency by merely local treatment where the pain last appears, is unreasonable and usually unsuccessful.

The differential action of the galvanic and faradic currents has not always been distinctly kept in view. For true neuralgia in any part of the body, the galvanic current is usually preferable to the faradic. It is not true, however, that genuine neuralgia never yields to faradisation; it does so quite frequently; but everything depends on the *kind of faradic current used*, and the *method of application*. There is as much difference of therapeutical effect in the qualities of faradic currents as generated by different machines, as there is between the galvanic and faradic currents.

A rough, harsh current, such as is generated by Stohrer's machine, will almost always do more evil than good to neuralgia, especially in the irritation stage. Such a current will pretty surely do harm if it be applied strongly and recklessly, and may do no good when applied gently and cautiously. On the other hand, a rapidly interrupted and pleasant current, such for example as is generated by Kidder's machine, when applied cautiously, may, and in my hands frequently has, relieved and cured true neuralgia.

The physical principles that account for this differential action of the faradic currents from these different machines is somewhat complex and cannot here be discussed. I may suggest that the physiological and therapeutical action of faradic currents seems to vary, not only with the length and thickness of the coils, but with the rate and manner of interruption, and with the details of the construction of the helix. All the leading forms of neuralgia—facial, central, occipital, cervico-brachial, gastralgia, and sciatica—I have treated with more or less success by faradisation with secondary and tertiary currents, from machines such as I have referred to; and yet the galvanic current will oftentimes succeed where the best faradic fails; and even in those cases where faradisation relieves and promises to cure, galvanisation may do the work in a much shorter time.

In regard to the question of the differential value of large and small cells in neuralgia, I may say that the idea that large

cells send greater quantity of electricity through the body than small cells is opposed to the teachings of Ohm's law, and has no basis in experience. It is, however, not improbable that large cells, especially those various modifications of Daniell's, by virtue of their greater constancy and the great slowness and uniformity of the chemical changes that take place in them, may be less irritating, and therefore better adapted for neuralgia and other irritable conditions.

The difference in this respect cannot, however, be very great, since many of the greatest triumphs of the galvanic current over neuralgia have been gained by small and inconstant single cell batteries. The larger double cells are more steady in their action, but steadiness may not always be necessary even in testing neuralgia, and it is by no means impossible that in some conditions the very fluctuations of the single cell battery may have a therapeutical advantage. Certain it is, that many of the brilliant results in the treatment of diseases of the central nervous system by electricity have been obtained by portable batteries, composed of a moderate number of small, single, and inconstant cells of zinc-carbon or zinc-platinum.

The electric moxa—faradisation with the metallic brush—is a method of treatment which in my hands has not been successful. I have seen the method used for sciatica in Berlin and in France, and I believe that it sometimes accomplishes good things; but it is a most atrociously painful procedure, and I rarely find a private patient in America who will endure it, and for this reason I have almost lost the habit of ever attempting to use it.

Too strong currents have been used, and too much stress has been laid on the differential action of the poles or of current direction. Beginners in electro-therapeutics make mistakes in many directions, but in no one more than in the habit of using too powerful currents. In many diseases and in many temperaments strong currents are harmful, but they are especially so in neuralgia. Better to use a mild current a long time than a strong current a short time; electro-therapeutics cannot be thus concentrated. To attempt to gain time in this way is cruel economy.

In regard to the differential action of the poles and of the current direction, I admit that of course there is a differential therapeutic, as there is a differential physical and physiological action of the poles, but it is far less marked in therapeutics than in physics and physiology. The importance of direction of the current, whether ascending or descending, has been enormously overrated, not only in neuralgia but in all the diseases for which electricity is employed. Pain is relieved, neuralgia cured, and the nutrition in every respect improved, by

the passage of the current through the body by either pole and in any direction, and the differential action of the poles and of the current direction is so slight and so complex that it cannot in the present state of physiological and pathological knowledge be reduced to any formula. I have elsewhere laid down the very general law that in therapeutics the positive pole is less irritating; but even this general statement seems to have many exceptions, in local application at least, for the negative will frequently relieve an irritated nerve as well as the positive, if not better. In central galvanisation and general faradisation, I use, as a habit, the positive pole in making the application to the head or neck, believing this to be, on the whole, the safer.

The treatment has not been used with sufficient perseverance. Perhaps by the use of too strong currents the pain is at first increased; the patient and physician become alarmed and abandon the treatment. This is frequently a mistake; the fault is in the method, not in the medicine. Even when the pain is increased temporarily the permanent effect may be beneficial. Neuralgia is frequently a chronic disease, and like other chronic diseases demands chronic treatment. Neither patients nor physicians should be terrified or discouraged because the first applications appear to aggravate the attacks.

With these general remarks I proceed to speak of some of the special forms of neuralgia.

Migraine.—The prognosis of migraine under electrical treatment is favourable for relief and palliation, but to thoroughly and permanently eradicate the habit is more than we can generally expect. Even relief of the pain during the height of the attack is by no means a uniform result. Here, as everywhere in electro-therapeutics, as much depends on the temperament of the patient, or his general susceptibility to electricity, as on the special disease from which he suffers. The attacks may be forestalled or aborted at the beginning by electrical treatment, just as they may be by bromide of postasium, or sodium, or hydrate of chloral, or gelsemium; but even here there is no uniformity.

In some cases the application of a very mild galvanic current to the head or neck at once causes the pain to depart, and it does not return. In other cases the same treatment does no good, and sometimes intensifies the pain. I have been at different times a victim to migraine, and have tried galvanisation before the attacks, while they were coming on, and at their height, and never with success. I have friends similarly afflicted, whom the same treatment for the same kind of attack always more or less relieves.

I think I have observed that migraine, accompanied with

gastric disturbances, is more rebellious than when the disease is confined to the head.

Although electro-therapeutics must be based on clinical experience, it is yet an advantage to have some general notion of the pathology of the disease that we treat; and when, as in the case of migraine, we cannot demonstrate the precise nature of the morbid changes in the nervous system that give rise to the symptoms, it is yet desirable to obtain, or to endeavour to obtain, clear and correct ideas of the seat of the disease, even though we can go no further. Our method of applying electricity will be guided, though it should not be controlled, by the theories we hold or the facts that we possess, in regard to the pathology of the disease for which we employ electricity.

Without entering into an extensive consideration of the nature of migraine—a subject which has been well and variously discussed in a series of articles in the Practitioner—I may simply say, in brief, that I regard the disease as a neuralgia closely related to facial neuralgia and gastralgia; that it may be excited by mental influences, by exhaustion, or, reflexly, by digestive or genital disturbances; that nausea and vomiting are the results and attendant symptoms of the disease, rather than the disease itself; and that the seat of the disease is in the pneumogastric and fifth nerves at their origin.

Acting in a measure on these views, which are based on long study of the affection, I treat migraine by my method of central galvanisation, which consists in placing *the negative pole to the epigastrium (the patient holding it by an insulated electrode), while the positive is applied on the top of the head, over the sympathetic in the neck, and down the whole length of the spine, in such a way as to bring the brain, the pneumogastric, the spinal cord, and all the prominent plexuses of the sympathetic, indeed the whole central nervous system, under the influence of the current.* This method is to be used in the intervals of the attacks, with the view of preventing their recurrence or of lengthening the intervals. During the attack, galvanisation of the brain or of the cervical sympathetic and pneumogastric in the ordinary method may be used for merely temporary relief. Faradisation from the machine, that gives a pleasant current, with the hand as an electrode, sometimes acts as well as galvanisation. In all these applications mild currents should be used—so mild as to be scarcely felt, even when the sponges or flannels are saturated with salt water.

It is not indeed necessary that the current should be felt at all, so long as we are sure it is running, and the current should be gradually put on or off. Acting with these precautions, galvanisation of the brain, or of the *neck, is an absolutely safe procedure.*

The mistake of electro-therapeutists is, to apply to the head or neck currents as strong, or nearly as strong, as those that they apply to the muscles in the treatment of paralysis.

During the past few years I have made many thousand applications of the galvanic current to the head and neck of patients of nearly all ages and of every degree of delicacy and susceptibility, without producing any dangerous or alarming effects whatsoever. In two or three instances, through my carelessness in suddenly interrupting the current, unpleasant, though not serious, effects have occurred, which soon passed away.

In using central galvanisation I sometimes move the negative electrode down over the abdomen and the hypogastric region, the positive being on the back, so as to bring the uterus and ovaries under the influence of the current. In cases where there is reason to suspect that the migraine is excited by uterine irritation, systematic electrical treatment of the uterus and ovaries, internally and externally, has been of advantage.

Facial Neuralgia.—This formidable disease yields to electrical treatment only in a certain proportion of cases. The brilliant results recorded by Niemeyer and Reynolds are rare. The milder cases, in patients not too advanced, and brought on by exposure to cold, sometimes are relieved and cured both under faradisation and galvanisation. The more severe and long-standing cases in the very aged may be relieved, but are not usually cured. The exceptions to the statement are, however, sufficiently encouraging to make us try all cases that come under observation. Galvanisation of the affected nerve-branch and of the brain, in various directions, and galvanisation of the cervical sympathetic, are the methods that I employ in this disease. This is one of the forms of neuralgia where it is very easy to aggravate the pain by an incautious use of electricity, and where there is especial need of patience and firmness. The temporary aggravation may be followed by relief, and the patient should not be allowed to abandon the treatment simply because the pain is made worse during the first week of treatment. Patients should be taught that they must give to electricity the same respect that they give to other remedies, and allow both it and the physician who employs it a fair and honest trial. The chances for permanent cure of facial neuralgia of long standing in the aged are perhaps not more than one in ten, if indeed they are as good as that; but such a chance is enough to stimulate our perseverance in every case that appears.

Gastralgia.—The form of neuralgia in which I have found the best results from electrical treatment is gastralgia. Cases where the pain is prevalent and of a most agonising character have been not only relieved for a few hours or days, but per-

manently cured, so far as can be seen now, by central galvanisation, which is *par excellence* the method of using electricity in this affection.

The two important facts connected with the prognosis of gastralgia under central galvanisation, judging from experience up to the present date, are that very long-standing cases are cured, and the relief is permanent. Two years ago I treated two patients—both physicians—for gastralgia of the most severe character: both were rapidly relieved and completely cured, and both are well to-day. In one of the cases the disease had existed for fourteen years, the attack coming on periodically about two o'clock every night.

In another patient, who for twenty years had suffered from gastralgia and angina pectoris, the cure after a long course of treatment has also been permanent.

Possibly I have been peculiarly fortunate in my cases of gastralgia; but if I were to generalise from my own observation, and from the observation of some of my friends who have employed central galvanisation in this affection, it yields better and more permanently than any other form of neuralgia.

Faradisation, however administered, does not accomplish all that we desire in gastralgia; it relieves, but does not entirely cure. The cases above referred to had all been treated by faradisation, in one instance with some benefit, but not with satisfaction. Galvanisation of the cervical sympathetic and pneumogastric may be used for gastralgia, but these methods have never been my dependence in any form of neuralgia.

Sciatica.—The prognosis of sciatica under electrical treatment, however skilfully administered, is not so satisfactory as is the prognosis of several forms of neuralgia. My experience with this disease does not accord with the highly favourable reports which have been given by various writers.

There is a certain proportion of cases of sciatica that yield readily to the soothing influence of the galvanic current, and after a few applications recover entirely; but such cases are not the rule, although the soothing, quieting, relieving effects are so common as to be almost always experienced. The pain gives way to a proper application of galvanism very frequently during the course of the sitting, yet there is pretty sure to be a recurrence of pain in the course of a few hours, and a very long course of treatment is necessary to make a permanent cure, and there is a certain proportion of cases that never recover, however perseveringly the treatment may be carried out.

There appear to be two reasons why sciatica is thus obstinate.

1. The nerve is larger and longer than any other nerve of the body, and the extent of surface to be treated is, of course, much greater than in other forms of neuralgia.

The disease would appear to be in part peripheral, at least in those varieties that are excited by rheumatism, if not in all the varieties. This view is rendered probable by the very great tenderness, and by the fact that pain is excited by pressure, as in sitting, or by any constrained position of the muscles that cause irritations of the nerve.

2. The nerve, especially near its origin, is so deeply situated that only a very small portion of the current, in external applications, can directly reach it.

My method of treating sciatica is to place one pole—it matters not which—on the spine, near the origin of the nerves that supply the lower limbs, and to pass the other slowly and steadily up and down the whole course of the nerve in the leg, giving especial attention to the tender points. Sometimes I place one of the poles in the hollow of the foot, fastening it there by a cloth or band; but I am not aware that there is any advantage in that arrangement.

I use a current that gives a slightly burning feeling without being positively painful. This method of application will relieve for the time, perhaps for several hours, or for an entire night or day.

Recently I have attempted the treatment of sciatica by *electro-puncture*. The needle, either insulated or not, should be bayonet-shaped, so as to go in easily, and should be inserted by preference at some tender point. I generally try to insert it far enough for the point to touch or penetrate the nerve. The moment when it so touches or penetrates will be revealed to the patient by a tingling or pricking sensation through the leg.

I connect the needle thus introduced with the negative pole of a mild galvanic current, two, four, or six cells, while the positive is applied at the back or some indifferent point. The application should not be made long enough, nor should the current be strong enough, to cause much electrolytic action. It is my custom to use this method for a few moments and then allow the needle to remain *in situ*, to resume the external application for a few moments, closing the sitting by again connecting the needle with a mild current. Thus I combine electro-puncture with the old treatment of acupuncture. I am inclined to think that this method of treatment is more rapidly and more permanently efficacious than external applications. It concentrates the current at the point where it is needed, and

perhaps may also act as a counter-irritant. It is probably better that the needle should be insulated up to within a short distance of the point, but I have frequently used uninsulated needles.

The great practical difficulty with electro-puncture is that the introduction of the needle is painful and full of terrors for the sensitive patient.

This difficulty may be overcome in various ways: by inserting the needle quickly and rapidly without forewarning the patient, by using a mixture of carbolic acid and ether, equal parts, or ether spray, to produce local anæsthesia.

But there are some patients who so dread the thought of the needle that they will suffer anything before resorting to it. Two cases of this sort have recently fallen under my observation. One of these was a physician, who is himself accustomed to the use of the electro-puncture, and who has had excellent success with it in sciatica. He had become so reduced by the disease, that when I was called in he refused to be treated by electro-puncture.—*Practitioner, Sept.* 1873, *p.* 187.

29.—CASE OF TRIGEMINAL NEURALGIA TREATED WITH THE CONSTANT CURRENT.

By SAMUEL CRADDOCK, Esq., Shepton Mallet.

[The patient was 64 years of age, and was seized somewhat suddenly with severe neuralgia of the left fifth nerve. The foci of pain were situated over the malar bone and at the junction of the nasal cartilage and nasal bone; also on the brow, over the supra-orbital foramen.]

The immediate exciting cause of this attack appeared to be that the patient had stood at an open window during some damp weather that was prevailing at the time. This gentleman had been the subject of severe albuminuria of long standing; it had in fact existed for many years, but there was no evidence of breaking down of the kidney-structure. At the time of the attack the urine was, moreover, very acid, and contained many uric acid crystals; sp. gr. 1025.

The treatment at first consisted in warm fomentations to the face, and the internal administrations of alkalies with mild alterative aperients: after a few days he seemed to improve, and the medicine was changed for a mixture containing iron, arsenic, and nux vomica. On October the 26th he went to town and consulted his London physician, who regarded the

attack as due to the gouty diathesis, and put him anew, upon alkaline treatment. The patient returned to the country on November 19 very much worse; in fact, I never saw anyone suffer so severely from neuralgia. The lachrymation from the left eye was excessive, and there was a copious flux from the left nostril; the nostril was so exquisitely tender that he could not bear to use his pocket-handkerchief. He complained also of severe shooting pains passing through the interior of the eye. These symptoms steadily increased in severity, and the branches of the posterior dental nerve, which are distributed to the gums, became affected, as well as the middle dental branches of the supra-orbital: this was indicated by pain referred to the site of the antrum. All the cutaneous branches of the superior maxillary were now alive with pain, and another focus was developed in the upper lip. The sensory part of the inferior maxillary, distributed to the lower lip, followed suit; and at last, so far as one could see, every branch of the sensory portion of the left trigeminus was more or less affected. The patient became perfectly unable to eat anything at all solid, and lived for two months entirely upon liquids: even these he was only able to take through a reed or a glass tube. It is scarcely necessary to enumerate the long list of remedies which were employed; they included the hypodermic injection of morphia and atropia, but the patient could not tolerate these, and the hypodermic method consequently did not obtain a fair and complete trial. The first remedy that gave any appreciable relief was the external application of aconite liniment, which was done morning and evening, and certainly mitigated the pain: meanwhile, however, the lachrymation and the defluxion from the nose continued in full force, as also the tenderness of the parts. Early in February 1873 I applied the constant current, employing a 50-celled Weiss's battery. The current was derived from eight cells, and was applied for a period of five minutes at each sitting. The positive pole was placed at the back of the neck, and the negative applied successively to the several foci of pain, being kept steadily on each for a minute or more. This was done daily, at the same hour; after the third application the lachrymation had nearly ceased, and the patient was able to blow his nose freely. From this time the improvement was continuous; mastication was performed easily, and at the end of a fortnight the cure was complete. The patient could not avoid perceiving the remarkable influence of the galvanism, and spoke of it, with the greatest delight, as a "perfect cure." My own satisfaction was equally great, as I had previously, from inexperience of its use, been inclined to doubt the efficacy of the constant current. This case certainly speaks strongly in favour of this

method of treating neuralgia, and incidentally supports the pathological theory of neuralgia advocated by Dr. Anstie in his work on that disease.

[The above case interests me greatly: I regard it as a typical example of the mistakes that frequently result from the time-honoured doctrine that neuralgia is usually produced by the gouty diathesis. So long as that theory was acted upon in this case, no really effective steps were taken towards the arrest of the disease, and a very little more continuance in the same course would not improbably have rendered the malady incurable, considering the patient's advanced age. I understand from Mr. Craddock, that there was really no positive reason for supposing that this patient had gout in him: and certainly the mere presence of an excess of uric acid in the urine is no justification for such an assumption. The first remedy that made any impression was the linimentum aconiti, a local application which can only have acted in one way, viz. by temporarily paralysing the peripheral portions of the nerve, and thus blocking the channels by which impressions from without were pouring in upon the irritated nerve-centre. Aconite belongs to a small class of remedies which are often of great service in procuring a momentary pause, during which neuralgic nerves have time to recover their equilibrium a little, while more radical curative measures can be devised.

As regards the action of the constant current, there can be no reasonable doubt that this was genuinely effective, and I beg to call attention to the fact that the direction of the current was what is called "inverse," viz. from centre to periphery. This is contrary to the method strongly advocated by many writers on medical electricity: but the result corresponds with that of inverse galvanisation as seen in several cases under my own treatment. I may especially advert to the case of a footman (described in my book on Neuralgia) in whom an exceedingly severe facial neuralgia of the first and second divisions of the fifth was found to be perfectly under the control of the constant current, and the effects of inverse and of direct galvanisation were found to be practically identical. I take this opportunity of remarking that evidence is fast accumulating which tends to show that the older ideas as regards the influence of the direction of the current must have been to a large extent erroneous. The opinion of Dr. Reynolds and of Dr. Buzzard coincides with my own upon this matter, and has been arrived at, equally with my own, by sheer experience, in the face of unavoidable prepossessions in favour of the usual ideas as to the respective effects of the two currents. —F. E. ANSTIE.]—*Practitioner, June* 1873, *p.* 338.

30.—SOME OBSERVATIONS ON THE USE OF PHOSPHORUS IN NEURALGIA.

By J. Ashburton Thompson, Esq., Surgeon-Accoucheur to the Royal Maternity Charity.

[Although there had been previously allusions to the subject, we are really indebted to Mr. G. M. Bradley, of Manchester, for drawing the attention of the profession to the use of phosphorus in neuralgia. (See *Retrospect*, vol. lxvii., p. 51), for three cases successfully treated by Dr. Broadbent with this remedy.]

The following remarks bear exclusively upon cases of nerve-pain, and these have been carefully differentiated from similar diseases. They are arranged in three classes, which are named respectively, acute primary attacks, acute recurrent attacks, and chronic cases. Of the first class there are only six cases, but it is almost characteristic of neuralgia to recur, and it will be interesting to note whether some of the rapidly-cured primary cases are liable to anything like an early return of the disease. The third division of six chronic cases do not present such perfect results as the two former classes; still, if it be remembered in considering them, that two were complicated with phthisis—one being advanced in that disease—while a third was already reduced to a dangerous condition of debility before coming under treatment, I think it will be readily allowed that the result is on the whole eminently satisfactory. In the tables setting out these cases, the conditions and events of them are classified under headings which speak for themselves. Under "Complication," if debility or anæmia be inserted, it has been of a very marked kind. Occasionally the time occupied by the cure seems disproportionate to the duration of the disease; but in all cases ultimately benefited relief followed the first few doses, although the cure could not fairly be considered complete until all soreness or numbness—all symptoms of disturbed sensation—had been removed.

Dose of Phosphorus.—The dose of pure phosphorus is stated in the British Pharmacopœia to range from $\frac{1}{30}$ to $\frac{1}{10}$ of a grain. Dr. Radcliffe and Dr. Broadbent have used the former dose with success; Dr. Anstie with little or none. Dr. Adolphe Waltuch, in his "Dictionary of Materia Medica and Therapeutics," gives the dose as being from $\frac{1}{20}$ to a $\frac{1}{4}$ of a grain.

The last quotation alone gives the proper range of dose of this remedy. To prescribe less than $\frac{1}{20}$ of a grain in the first place is to render its therapeutic action apparently variable or uncertain. In some of my earlier cases I am of opinion that recovery was unduly delayed in consequence of the small quantity with which treatment was begun; at least it will be observed that alleviation of the symptoms commenced much

sooner in those cases last under my care. I now invariably begin by giving $\frac{1}{15}$ of a grain every four hours, and this I conclude to be an average dose; it has proved itself a perfectly safe and very efficacious one. In only one case will it be observed that any serious symptoms arose which might perhaps be attributed to the remedy, and in this instance $\frac{1}{15}$ of a grain was the quantity prescribed. The symptoms appeared, however, to be in a measure critical, and, since they have on no occasion repeated themselves, may have rather been induced than originated by the medicine employed. On the other hand, another patient arrived at taking by accident $\frac{1}{4}$ of a grain every four hours, and continued to do so for some days, without harm. On consideration, I conclude that to be on the safe side; and at the same time to secure the proper therapeutic action of the drug, treatment may commence with $\frac{1}{15}$ of a grain repeated every four hours, which should be continued for six doses. If there be now only slight or no alleviation of the symptoms, the dose should be increased to $\frac{1}{15}$ of a grain, and continued at similar intervals, After the latter quantity has been taken for forty-eight hours, I am inclined to think that some good may be effected—*i.e.*, if cure have not already resulted—by increasing the dose still further, either in quantity or frequency of administration; but I do not think that any good will be effected by a larger dose of which $\frac{1}{15}$ of a grain has not given decided promise. I have not, however, had a sufficient opportunity of investigating the action of larger doses.

Of the Mode of Administration.—The mode of administering pure phosphorus is of the first consideration, for two reasons: the ease with which it alters under exposure to certain bodies, and the extreme nauseousness of the fluid preparations.

Phosphorus may be dispensed from its solutions in oil, in ether, in chloroform, in spirit; or it may be given enclosed in capsules or pills (Radcliffe, Broadbent); or in loose chemical combination, as in the hypophosphite of soda (Radcliffe, Anstie), or phosphide of zinc. In the course of the experiments alluded to above, conducted in 1868, M. Dujardin Beaumetz was led to reject the solutions in ether and chloroform, on account of the deleterious effect of the solvent in repeated doses. Spirit he rejected on account of the small quantity dissolved by it—one part in 320, at a specific gravity of ·799. Nevertheless, this is the preparation which I have chiefly used, and *I find that, with the aid of heat, ten grains of phosphorus dropped into* 1,000 *drops of absolute alcohol do disappear, and are not again precipitated.* M. Beaumetz selected finally *oil of sweet almonds* as the best solvent. This preparation must either be made into an emulsion, in which form it is extremely

nauseous, or it may be enclosed in capsules. These were found, however, to deposit after a time an insoluble, and therefore presumably inactive form of phosphorus. The means of avoiding this is to employ oil which has been superheated, which will be found to make a clear and stable solution. No doubt if the capsules and pills now obtainable are guaranteed to be made with oil thus prepared, they afford an agreeable and perhaps sufficiently efficient means of administration; but I believe it is conceded that there are often advantages enjoyed by fluid forms of medicine which are either largely diluted in the first place, or which admit of easy and rapid solution in the fluids of the body. The pills as recommended by Dr. Radcliffe are made by heating phosphorus and suet together in a close vessel, in the proportion of one part to a 100, making into three-grain pills, and coating with gelatine. The oily solution is a preparation of the French codex (phosphorus 2, oil of sweet almonds 100; dissolve with heat), and of the Prussian (phosphorus 1, oil of sweet almonds 80; dissolve with heat). A French formula for emulsion (Soubeiran) is, phosphorus oil 8, gum arabic 8, peppermint water 100, syrup 60; make an emulsion.

The four successive cases related by Dr. Radcliffe were treated with the hypophosphite of soda in doses of from seven to ten grains three times a day; and Dr. Anstie has used the same quantity of this remedy in similar cases of nerve-pain, in which he did not find it successful, although he was satisfied with its effects in cases of chronic alcoholism. It is said to be not unpleasant to take. Phosphide of zinc contains one-fourth of its weight of pure phosphorus; but two parts of the compound must be administered in order to produce the effect of one part of phosphorus. I have no experience of these forms. Dr. Anstie has used pills containing $\frac{1}{33}$ of a grain of phosphorus; these were made according to Dr. Radcliffe's formula; he was dissatisfied with the result. In two cases I used coated pills containing the same quantity, and made by Messrs. Kirby and Co.: they appeared to be without effect, although I gave twice the dose administered by Dr. Anstie, and was afterwards successful with the remedy in other forms. Dr. Broadbent used capsules in his three cases of nervous disorder with good effect.

The discrepancy may now be noted between the results obtained by two competent observers with the hypophosphite of soda used in similar cases. Is this an unstable compound? Again, the results obtained from capsules are opposed to those obtained with pills—which, I believe, are of the same or similar composition. Can any such deposit of insoluble phosphorus have occurred in the pills, such as is indicated by M. Beaumetz?

I have used the oil (with the action of which also Dr. Anstie was not pleased) in five cases—exclusively, in three. The formula employed was—

Formula No. 1.—Phosphorised oil, 3 drachms; powdered gum arabic, 6 drachms; spirits of peppermint, ½ drachm; water to 6 oz.

I have avoided the use of syrup (as recommended by Soubeiran), or of chloric ether, both of which increase the nauseousness of the mixture. As tincture of orange is the best vehicle for chloral, so spirit of peppermint is incomparably the best vehicle for phosphorus. This mixture keeps reasonably well, and is, I am inclined to think, a very active preparation—perhaps more active than any other. It is, however, so extremely nasty, and, moreover, seems of itself to disagree so with the stomach, that I have not more than one patient who professed to have retained more than fourteen doses.

Tincture of phosphorus should be made by dissolving the material in absolute alcohol with the assistance of heat. The most convenient proportion for dispensing is:—phosphorus, one grain; absolute alcohol, three drachms. This mixture will, I know, retain its powers for six weeks, although not kept in a full bottle; I do not know that it loses its efficacy after this period. Before adding this tincture to water, it is necessary to dilute it still further with rectified spirit; otherwise hydrated phosphorus will be precipitated. The formula I have employed is the following:—

Formula No. 2.—Tincture of phosphorus, 3 drachms; rectified spirit, 2 drachms; spirits of peppermint, ½ drachm; water to 6 oz.

This forms an active, efficient, and convenient preparation. It is not quite disgusting, and will be readily taken and well tolerated by those who have begun to experience benefit from the first few doses. It is unstable, and should therefore be supplied to the patient in quantities not calculated to last more than twenty-four hours. I have used this mixture in the majority of cases.

Observations.—The first general observation which presents itself is, that chronic cases are cured the least rapidly when they are cured at all; while among them is found the only absolute failure of the remedy employed. This obstinacy may be referred to one of the following causes:—First, the habit of pain into which the nerves have got, and the reaction upon their functional activity which the debilitating effect of constant pain must have; or, secondly, the extreme or particular kind of debility to which the complicating disease has reduced the subject; or progress of a specific nerve disease beyond the possibility of rapid recovery; or, lastly, the necessity for time to recover from the condition of anæmia which in a particular case may be the prime cause of the symptoms. For example,

one case was complicated with phthisis, which was advancing slowly but distinctly, and in addition was the true subject of a great deal of depressing mental irritation. Almost constantly for the last eighteen months she had been subject to nerve-pain affecting the cervico-occipital and brachial nerves, and, on bad days, both trigeminal nerves. She was under observation altogether for about three months, being treated in the first place with every kind of remedy, including muriate of ammonia. In my experience this remedy, administered in doses of from one to two drachms every two or three hours, ranks next to phosphorus in efficacy; but it must be given in doses as large and frequent as these; and in those cases which require the remedy —in neuralgia—I do not remember ever to have found it productive of sickness. Finally, since the attacks recurred with added severity every third day, she took large doses of quinine without effect. She was now put upon $\frac{1}{16}$ of a grain of phosphorus dissolved in oil, and she derived some relief from the nine doses which she was able to retain; after that the stomach rejected the mixture. She then took $\frac{1}{12}$ of a grain every four hours in F. No. 2 for about five weeks, when she removed to the country. She was considerably relieved, but not by any means cured.—*Practitioner, July* 1873, *p.* 14.

31.—THERAPEUTICAL VALUE OF PHOSPHORUS.

In an interesting paper on the physiological action and therapeutical effects of phosphorus, published in the Bulletin Générale de Thérapeutique, M. Gubler states that phosphorus is a diffusible stimulant of great energy and of dangerous activity. It should therefore only be prescribed with the greatest possible caution, and certain contra-indications may in the first instance be laid down. Thus it should not be used in any affection characterised by nervous, circulatory, or trophic excitation, as in tonic and clonic convulsions, contractions, neuroses having a hypersthenic origin, diffuse peri-encephalitis with general paralysis, phlegmasiæ of all forms, fevers of every kind, exanthematous affections, &c. The indications for its use are the existence of disease unaccompanied by inflammation, fever, and nervous excitation, and especially in such cases as are characterised by depression of the circulation, either local or general, diminished power of generating heat, exhaustion or local asthenia, with paralysis of sensation and movement. Hence its value in cachectic states consecutive to long and exhausting diseases, marsh fevers, protracted convalescence, tabes dorsalis, paralyses of old date, and of cerebral, medullary, or peripheric origin, when there are no signs of irritation; in hemiplegia, paraplegia, amaurosis, and other

partial paralyses. Phosphorus again is sometimes useful in making chronic eruptions advance or recede; but it is especially as a remedy for impotence that it has been praised, though it has often disappointed the expectations of those who have prescribed it, and has either proved of no value at all, or its effects have only been ephemeral. Hence it would appear that the real remedial power of phosphorus is considerably restricted, and that it can only be regarded as of great value in paralytic affections. Dr. Delpech, who has studied so deeply the effects of sulphuret of carbon, praises phosphorus highly as an agent to remove the paralysis and loss of power which accompany intoxication by that substance. It is also of service in the so-called rheumatismal and hysterical paraplegiæ, or in other words those forms of paraplegia which are not caused by organic lesion, as well as in cases of cerebral disease in which all irritation has ceased and cicatrisation has taken place. It is still more strongly indicated in cases of asthenic and diffused paralysis, consequent on diphtheria or some other acute affection. Gueneau de Mussy, Isambert, and Féréol have all found it efficacious in the treatment of mercurial tremor; it is also believed to be so in paralysis agitans and in the various forms of medullary sclerosis affecting the antero-lateral cords, and above all in sclerosis of the posterior columns, the symptomatic expression of which is summed up in the term locomotor ataxy, which has been applied to it by Duchenne. Dujardin-Beaumetz is he who has most strongly recommended the plan of treatment by phosphorus in this and similar affections; but it is questionable M. Gubler thinks, how far many of the successes attributed to its uses are really due to its remedial powers. We forget, he says, the natural processes of cure that often take place. Phosphorus is an active agent that may momentarily re-illumine the fading spark and revivify the languishing powers of life; but as it brings no energy with it, it impoverishes rather than enriches, and can do little for a nervous system exhausted by a chronic affection. The amorphous phosphorus is perhaps the best mode of prescribing it, as this possesses no exciting or irritating action. Externally, it has been chiefly employed in squamous affections of the skin as a parasiticide in itch, and as a caustic in the place of the moxa. Recently Tavignot has declared that it will render the cataractous lens transparent, but the negative facts obtained by MM. Gosselin and Maisonneuve render this more than doubtful. In regard to the mode of its administration, solutions are usually preferable to pills. Amongst the former are the ethereal tincture, which contains one part in sixty, and of which ten drops are a dose; the solution in chloroform, which is now almost abandoned; and

the solution in oil, which is by far the best, and especially that prepared by Méhu. Here the oil is dehydrated and decolorised by exposure to a heat of 250° C., and the phosphorus is added when it has cooled. A twentieth part of ether is then added. The proportion is one part of phosphorus to 500 of oil, and it contains two milligrammes in fifteen drops. Each capsule contains one milligramme.—*Practitioner, July* 1873, *p.* 46.

32.—SLIGHT POISONING FROM THE MEDICINAL USE OF PHOSPHORUS.

By Dr. ANSTIE.

At the present time, when attention is being widely directed to the efficacy of phosphorus in nervous disesses, it may be useful to record every incident that throws light on the as yet undecided question of dosage.

A gentleman, aged 33, single, applied to me first in May 1872. He had been subject to migraine once a month (or oftener, under circumstances of special fatigue) in boyhood; but the pain was more or less diffused, and not very evidently neuralgic in type, till the age of nineteen. Since the latter date it has been strictly confined to the left side of the head (anterior two-thirds); occasionally it extended into the globe of the eye, and sometimes slightly invaded some descending cervical twigs. It never was felt in either maxilla.

The family history was decidedly bad. A brother was neuralgic; the mother died mentally deranged; a sister is partly insane.

Up to December 1871 the attacks of pain were always attended with vomiting; in that month there was a very bad attack of typical sick-headache, and then the vomiting altogether ceased to occur. Before the change just mentioned, the attacks were always preceded by vertigo; since that date the vertigo, like the sickness, ceased to appear. There never has been any marked lachrymation; but it is noteworthy that since the attacks have become more like ordinary neuralgia than migraine, the tendency to conjunctival congestion has markedly increased. On questioning the patient as to the nature of the vomiting that formerly occurred, he was quite clear in the statement that no solid food was ever thrown up; the sickness always took place in circumstances of fasting, and the vomited matters were watery and sour. He has not been blessed with a very good appetite; but, on the other hand, there is no history of anything that can be called dyspepsia.

As regards the remedies which had so far been employed, he had taken (among other things) quinine, iron with strychnia,

and also arsenic; the latter did him more good for a time than anything else. He had for some years been in the hands of a very intelligent practitioner, and had not suffered from the injudicious kind of treatment to which, unfortunately, migrainous patients are too often subjected.

In advising on this case, it became important to consider the circumstances of the patient's life. He was engaged in a business which often occupied him for many hours in a chiefly sedentary manner, and had likewise evidently undergone much anxiety and worry. I suspended, for the time, the question of his taking an entire leave of absence from business for a prolonged period, but insisted on his taking things more easily. He was recommended to take a steady course of arsenic and cod-liver oil, and to increase his allowance of food.

Early in May of this year the patient's ordinary medical attendant called me to him in consultation. It appeared that he had been better, on the whole, during the past twelve months; still, he had not shaken off the neuralgic tendency, and it became a question whether any further medication should be tried, or whether he should at once be sent on a prolonged sea-voyage. I was anxious to give phosphorus a trial; and on May 5th, accordingly, our patient commenced taking, thrice daily, a pill containing $\frac{1}{30}$th grain. On July 8th his medical attendant brought him to my house, with the following interesting history:—

From May 5th to May 11th or 12th he took the pills regularly, but during the two last days suffered from a constant and increasing burning sensation at the epigastrium. What led to the suspension of the phosphorus was the fact, to which the patient himself drew his medical man's attention, that the urine was dark-coloured: on examination it proved to be both albuminous and bloody, but very careful microscopic examination failed to detect any casts. After a day or two's interval the renal symptoms had almost disappeared, and the stomach was much easier, though the patient felt (and, indeed, still feels slightly) a rather burning pain between the scapulæ. As to the neuralgia, this is remarkable:—On the whole he had suffered more during the phosphorus-medication than previously, but it was evident that some powerful influence had been exerted on the nervous system, for the rhythm of recurrence of pain was quite changed. There were now two exacerbations daily, but the pain was altogether more of a continuous and less neuralgic type. On the 14th, after two days' rest from the phosphorus, he took one-five grain dose of quinine about an hour before the morning exacerbation was expected, and, singularly enough, never had a recurrence of actual pain afterwards, though there were a few occasional threatenings.

Hé went to St. Leonard's for a little while, and this seemed to help him; and on the 8th July, when brought to me, he was quite free from his neuralgia; but, though fatter than he had been, still undeniably thin. A stranger to the history of the case might suppose that the single dose of quinine had been the cause of the cure; but this seems impossible, for he had been saturated with quinine by his own medical attendant (before I saw him first) without the slightest benefit. At any rate one may say that the quinine would probably not have done him any good, had not a strong impression been previously made by the phosphorus.

I think that it is very evident that poisoning by phosphorus did occur in this case. The burning pain in the stomach, together with temporary hæmaturia, at once relieved by discontinuing the medicine, seem to leave no doubt of this; and thus we must acknowledge it possible that a patient may suffer the slighter degrees of phosphorus-poisoning from less than three-quarters of a grain in minute doses distributed over seven days. At the same time, in view of Mr. Ashburton Thompson's use of the drug in much larger doses, for a large number of patients, without any bad result, we must suppose that the occurrence is quite exceptional, and due to an unusual susceptibility.—*Practitioner*, *Aug.* 1873, *p.* 103.

33.—ON CHOREA.

By Dr. FRANCIS E. ANSTIE, Physician to Westminster Hospital, &c.

By far the commonest form of ordinary chorea is that which we see in young children, say of eight or ten years. There is usually something delicate in the appearance of these patients; yet this is by no means always the case, and not a few of the sufferers have actually the appearance of blooming health. The choreic movements begin, in the majority of cases, on one side of the body, and most commonly in the arm. Moreover the left side is far more frequently the seat of the original attack than the right; and in the wards you will very frequently see choreic children sitting in a characteristic attitude —namely, grasping the left wrist with the right hand, to prevent the movements of the left arm. Gradually the movements spread to the leg of the side first affected; and often about this time slight facial grimaces, and momentary jerkings of the neck-muscles, with perhaps some little affection of the tongue-muscles, are noted. The tongue may be protruded with a jerk—a symptom which has been much dwelt upon, but is not so common as many writers would have us to sup-

pose. In a very large number of choreic cases there is no tendency at all to spasmodic protrusion, but rather to a partial disability in all the motions of the tongue. Stammering, which is a not unfrequent symptom, must be discussed at a later stage of this lecture. In a large number of cases the movements remain limited to one side of the body: this is called "hemichorea."

The convulsive jerkings, however, although they are the symptom which most attracts the eye, by no means indicate the whole extent of the affection even of the motor nerve-supply. If you test the power of any given muscle or group of muscles which are choreic, you will find it notably enfeebled, the degree of weakness corresponding pretty accurately to the amount of choreic affection. In the case of the leg (especially in hemichorea) this weakness is often plainly shown by the patient's dragging the limb in walking. It is remarkable that, as a rule, the symptoms of diminished power vanish simultaneously with the abnormal movements; though you will hear of exceptions presently.

When we are able to gather exact particulars of the illness from its beginning, we commonly find that the invasion was very gradual. It is true that violent emotion, especially fright, does often precipitate, or apparently even altogether produce, a choreic attack; but I doubt not that by far the most of these patients were already predisposed to chorea, and that many of them had actually had slight and unnoticed symptoms before the occurrence of the shock which is regarded as the exciting cause. Often we find that the system had been previously weakened by an acute illness, and most frequently this illness was acute rheumatism, the connexion of which malady with chorea is so important that we must presently investigate it separately.

In the simplest cases of chorea, such as we have just been considering, there is a very strong tendency to spontaneous recovery. A great number of these patients pass through all the stages of their malady in some four, six, or eight weeks, even if nothing be done in the way of medicinal treatment. But if, from one cause or another, the disease remains substantially unchanged at the end of three months, it must be reckoned (as Jaccoud well observes) among the obstinate choreas, and is likely to give much trouble unless we are able to interfere with it by some decisive treatment. It is especially unfortunate if chorea drags on towards the time at which puberty naturally begins. The already enfeebled nervous system, caught in the new storm which now agitates the organism, is nearly certain to relapse, and perhaps more severely than before, into the ataxic manifestations of chorea.

This brings us to the consideration of a second variety, in which chorea evidently takes its first start from the changed condition of things which has been brought about by the establishment of puberty; and I beg your particular attention to the important differences which separate this variety of the disease from that which has been already described. Chorea which first takes its rise in the turmoil of sexual development is not only far more likely to prove severe, intractable, or even dangerous to life, than the earlier disease, but has a considerable tendency to be followed by permanent damage to the nervous system, which may show itself in other than choreic phenomena. It is among this class of choreic patients that we first meet with those severe and alarming cases which are spoken of *par excellence* as *acute* chorea. In these the muscular movements are neither slight nor limited; on the contrary, they speedily involve the whole voluntary muscular system, and assume the most pronounced character. Limbs, head, and trunk are in perpetual and violent motion; walking or standing is impossible, and the patient can hardly be kept in bed without being fastened down. The mental agitation becomes excessive, and sleep is impossible. In the worst cases the incessant chafing of the limbs against each other, and against surrounding subjects, produces sores, inflammations, and even extensive suppurations, that add greatly to the misery and exhaustion of the patient. Meantime she (for it is almost always a girl) cannot swallow, and unless feeding can be regularly performed per rectum, the mere starvation will quickly prove fatal. Delirium, of a more or less maniacal kind, attends such cases towards their termination.

Besides these extremely severe cases, in which the tendency is towards a speedily fatal end, the chorea of sexual development occurs in less pronounced but still very intractable forms. Should there be considerable difficulties in the establishment of a regular and sufficient menstruation, and especially if there is a tendency to neuralgic ovarian dysmenorrhœa, the case is very apt to become complicated with hysteria; and, once the hysterical attitude of mind is superadded to the chronic ataxy of muscles, the patient is placed in a situation, in which the nervous system will have great difficulties in reacquiring a healthy equilibrium. I shall have something important to say presently, under the head of etiology, as to the extremely disastrous influence of conscious sexual irritation upon the choreas of early sexual life; and I may here remark that such irritation may induce puberty precociously, and that the unhappy children whose minds have been unnaturally turned thus early to sexual ideas are apt to have chorea, if at all, in a form which resembles that just described as pertaining especially to a later period.

There are two other incidents of sexual life which, though in a feebler degree than the development of the sexual organs and feelings, may provoke chorea—namely, pregnancy and the "change of life." It is not often, I believe, that first attacks of chorea occur under either of these circumstances, unless some powerful emotional cause be also at work. Usually the chorea of pregnancy and the rarer chorea of uterine involution, are mere reappearances of a tendency which had already manifested itself in childhood. But occasionally we meet with persons of neurotic temperament and family antecedents, in whom the state of pregnancy keeps the nervous system in so unstable a state that the occurrence of any physical, or especially any emotional, shock at once evokes choreic troubles. Occasionally it has happened that the choreic agitation was exceedingly severe, and either killed the patient or induced premature labour.

On the whole, then, we may clinically divide the choreas proper into two main groups—those of childhood and those of sexual life. (One might add a third group of cases, extremely small in number, which occur in advanced life, and are evidently a consequence, though an unusual one, of the degenerative processes peculiar to old age.) The choreas of childhood are mainly of a mild and curable kind, though with a tendency to relapse, sometimes two, three, or four times, but usually ceasing altogether before very long. The choreas of sexual life, are, as a rule, more deeply rooted in the nervous system, are apt to be more extensively complicated, and have in most cases a more serious prognosis than the former class.

The pathology and causation of chorea form one of the most puzzling subjects that medical inquiry has ever encountered. When we have stated the very obvious facts that chorea is a disease of some portion of the central nervous system, and that the circumstances evidently favouring its occurrence are all of them of a debilitating kind, we have said very little, as you will allow. It is something to know, in addition, that chorea is very strictly bound up with the inherited disposition to neurotic disease. But when we have got so far, we are confronted with the most formidable difficulties, which may be expressed by the following questions:—(1) Is the pathological seat of chorea always the same, and if so, is it spinal or encephalic? (2) Is the pathological process always of the same general nature, and if so, of what nature? (3) What is the degree and kind of connexion between rheumatism and chorea?

1. As to the pathological seat of chorea. The older belief, that chorea was dependent upon spinal cord irritation has of late years been pretty much set aside, more especially in this

country. Dr. Reynolds some twenty years ago brought forward a very able series of arguments to prove that the phenomena did not correspond with the idea of a spinal origin of the disease: he argued that the persistent *clonic* character of the spasms, the partial control which the will has over them, the cessation of the movements during sleep, and the fact that emotional changes and attempts at volitional action induce or aggravate the spasms, are totally opposed to the spinal theory. Dr. Broadbent followed this up a few years ago, by adding to Reynolds's arguments the following:—Tickling of the hand or sole of the foot does not exaggerate choreic movements—indeed, there sometimes seems to be a lessened reflex irritability. And, again, the more or less unilateral character of the affection, in such a large number of choreic patients, is highly inconsistent with the spinal theory. The whole argument taken together seems to point rather strongly to the view originally put forward by Reynolds, that the pathological seat of chorea is the sensori-motor ganglia at the base of the brain, the corpora striata and optic thalami.

To this opinion as to the seat of the pathological change, Dr. J. Hughlings Jackson also agreed, but, as I shall have to mention presently, he adopted a theory as to a uniform pathological process in the encephalic sensori-motor ganglia which is not endorsed by Dr. Broadbent.

Not to dwell in detail upon the various doctrines that have been expressed, I may mention that M. Jaccoud holds, and very ably states the opinion, that a certain portion of the spinal cord is the true seat of chorea. He remarks that chorea is a perversion of muscular co-ordination; and he expresses the conviction that there is no *single* centre of co-ordination, but that the "elements" for that function extend the whole length of the spinal cord, as far as the pons Varolii. It is this apparatus which, according to Jaccoud, is morbidly affected at some point in its length in the majority of cases. The few examples of chorea in which he will admit the possibility of a primary lesion in the sensori-motor ganglia are to be explained on the theory that the encephalic lesion only takes an indirect part in the production of the chorea.

2. As regards the nature of the pathological process which produces chorea, there have been various opinions. The number of opportunities for post-mortem examinations of persons (either actually or within a short distance of time) suffering from chorea is very limited. In a considerable proportion of those which have been made, no lesion whatever of any importance or significance has been discovered; though, of course, in many of the old examples there is the possibility that the observer was incompetent to discover the lesion. Until com-

paratively recent years the principal lesions which had been observed were congestions of cerebral or spinal meninges; subarachnoid spinal effusion; and (occasionally) patches of softening in the brain. But for some few years past one theory has occupied the principal attention of the scientific world—viz., the embolic theory of Dr. Jackson. This author supposes that chorea is linked essentially with embolic obstructions of some of the bloodvessels supplying the corpus striatum; the effect of this interference with the supply of nutrition being to create an instability of voluntary movement which is a minor degree of that complete interference which would be represented by paralysis. The embolic theory of chorea has attracted a remarkable degree of attention. It was elaborated in a most careful manner, and its author has connected it, in a very ingenious way, with similar explanations of other phenomena presumably dependent on lesions of the corpora striata and the neighbouring parts. Of late, however, it has been widely felt that this theory is incapable of universal application—that it can, at most, but explain a comparatively limited number of cases. There is one objection urged against it by Jaccoud, which, though stated perhaps in too absolute a manner, is highly important. Jaccoud says the chorea, when unilateral, is nearly always left-sided; whereas embolism of the cerebral arteries by far the most frequently occurs in the left corpus striatum or its neighbourhood, so that the resulting chorea should be *right-sided*, which is just what it is not, except in a minority of cases.—*Lancet, May* 10, 1873, *p.* 655.

DISEASES OF THE ORGANS OF RESPIRATION.

34.—ON THE TREATMENT OF ASTHMA.

By Dr. C. T. Williams, M.A., Physician to the Hospital for Consumption and Diseases of the Chest, Brompton.

The treatment of spasmodic asthma is by no means easy, and in a disease where the modes of causation are so varied it is difficult to lay down rigid rules. The grand principle should be: Avoid all exciting causes of the attack, and place the patient under conditions most likely to allay, and keep allayed, spasm. The treatment may be divided into *climatic* and *medicinal;* the former being, as a rule, vastly more important than the latter. We have to discover a climate in which the patient can *breathe*—no easy task, indeed, in a disease the course of which follows neither rhyme nor reason, and often seems to depend wholly on the idiosyncrasy of the individual.

The only safe guide to follow in obstinate cases is the doctrine of contrasts, and, whatever the climate of the locality be where the disease has been contracted, to boldly resort to its very opposite: should it be a damp one, to resort to a dry one; if a dry one, to a moist one; and if inland, to the sea, and so forth. High localities suit some asthmatics; sea air a few; cold places some; warm localities others; but the atmosphere in favour of which the strongest evidence exists is that of populous towns, and the more smoky and drier the town the better for the asthmatic. Trousseau and Sée give marked instances of asthmatics who were cured by quitting their country homes and residing in Paris. Salter enumerates many striking examples, and even concludes that the worse the air is for the general health, the better for the asthma. Thus he considers the worst parts of cities—*i.e.*, the least hygienic—the best for asthma; and, conversely, the best or most hygienic parts the least suitable for asthma; and in London he prefers the City to the West-end.

With reference to the advantages asthmatic patients derive by removing to the crowded portions of cities, a well-known physician of Nice furnished me with a good instance. An asthmatic English nobleman, who had resorted to the sunny south for sunshine and balmy air, took up his residence in a beautiful villa on the Promenade des Anglais, facing the sea. The situation was excellent, and the hygienic conditions as good as can be obtained at Nice; but he could not breathe, and spent night after night in great and breathless anguish. He had made up his mind to quit the place, and was one day wandering about the old town, a closely packed, ill-smelling neighbourhood, in search of a hair cutter, when he espied a small and by no means tempting-looking shop of the sort. He entered, and in the somewhat ill-ventilated, low-ceilinged room found to his astonishment that he could breathe with comfort. He gave up his grand villa, took up his quarters in the barber's first-floor, slept soundly, and ceased to wheeze.

Without going so far as Dr. Salter, I may say that I have found the air of London beneficial to by far the majority of my asthmatic patients, and I consider this the more remarkable because the localities they have quitted have been very dissimilar. Some come from high table-lands with little or no vegetation, others from deep valleys abounding in trees, some from damp river sides, some from the sea-coast, some from hot, some from cold climates. Even in town atmospheres asthmatic patients show themselves extremely sensitive to slight differences of situation; some can sleep in a back room and not in a front one of the same house, others in a garret better than in a first-floor, while a few feet of rise or fall in a city has been known

to make great difference. All densely-populated smoky towns appear to exercise this good effect; for instance, Manchester, Leeds, Birmingham, Glasgow, have furnished instances of asthmatics improving in their atmospheres with little aid from medicine. It has sometimes surprised me to witness severe attacks, which have lasted in the country for days and weeks, subside in a few hours in the much-abused atmosphere of our metropolis.

Now, what do we know about the air of cities? How does it differ from the pure air of the country? First, it is drier than that of most country places; secondly, it contains, according to Dr. Angus Smith's careful analysis, more carbonic acid; thirdly, it contains less oxygen. Dr. Smith made numerous analyses of the air in various parts of the metropolis, in each postal district, near the river-side, in the parks, in the most densely-populated portions; and in the subjoined table I have given some of the results which appeared to me to bear most closely on our subject. I have compared them with those of Glasgow, and, what is more to the purpose, with Dr. Smith's admirable analysis of specimens of air taken from the summits of nine or ten of the principal mountains in Scotland (the wind not being strong at the time of observation), as well as with the air of the Scotch seaboard and of open heaths of no great elevation.

	Carbonic acid, per cent.	Oxygen per cent.
Mountains of Scotland (average).....	·0332 ..	20·98
Seashore and open heaths ,, ...	— ..	20·99
Glasgow ..... ,, ...	·0502 ..	20·90
London..... ,, ...	·0439 ..	20·88
,, near London Docks (maximum)	·0528 ..	20·76
,, E. & E.C. Districts (average)..	·0474 ..	20·86
,, W. & W.C. Districts ,, ..	·0411 ..	20.92

This table shows a decided increase in the percentage of carbonic acid, as well as a diminution in the oxygen per centage, present in the atmospheres of Glasgow and London. The differences in the air of the various postal districts are much what experience would have indicated, the air becoming purer as we advance to the west in consequence of the large extent of parks and open spaces. The differences in the amounts of oxygen and carbonic acid, when taken in relation to the whole per centage, are by no means great, and are only to be found in the first or second place of decimals. But when we remember the small percentage of impurity which suffices to render drinking-water injurious in spite of the disinfectant power of the gastric juice, and the usually small amount of water swallowed, can we wonder that the lungs, in the course of respira-

tions 16 to 20 in the minute, and unprotected by any filtering apparatus, may introduce in the course of a whole day a sufficiently large quantity of a certain gas to have a decided influence for good or evil, even though the percentage of the said gas may be comparatively small. Dr. Angus Smith says: "We take into our lungs 1000 or 2000 gallons of air daily. The addition of $\frac{1}{2000}$ at each inspiration will give us fifteen grains in the day—an amount by no means to be overlooked." This excess of CO_2 in the London atmosphere, combined with the diminished amount of oxygen, may have a deadening effect on the pulmonary mucous membrane, and render it less sensitive to slight excitants. If it is not the excess of CO_2, or the smaller amount of O, it may be the unconsumed carbon, or some other component of town air which dilutes and renders it a less potent draught to the asthmatic lungs than the pure, fresh, champagne-like air of the mountains or open country.

The late Sir James Simpson was in the habit of having a certain amount of carbonic acid mixed with the atmosphere inhaled by asthmatics, and the baths of St. Moritz and Ems and others are sometimes used on account of the carbonic acid which the waters give off.

The great difficulty with regard to introducing a certain amount of carbonic acid into the atmosphere is the regulation of the quantity introduced, for too great care cannot be taken on this point, as a very small excess of carbonic acid may prove fatal. Martius concludes, from experiments, that carbonic acid is the principal result of the burning of the popular remedy of nitre paper, and if this be the case this is one of the safest methods of administering this gas.

The *dietetic* treatment of asthma must be to a certain extent arranged to each patient's particular case, but, as a general rule, the earlier meals should be made the fullest, and but little solid food taken after two o'clock. Stimulants should be of the kind least likely to cause flatus in the stomach and intestines.

The *medicinal* treatment of spasmodic asthma must to some degree depend on its causation, and, in fact, on how far the cases are simply neurotic, or complicated with, and dependent on, inflammation, or, again, on a morbid state of the blood.

All the local causes, with the exception of inflammation, produce a form of asthma which is a simple neurosis resembling neuralgia; but inflammation of the bronchiæ, as a rule, leaves behind it some organic lesion, against which it is necessary to direct our treatment if we hope for any success. It will often be found in these cases that the treatment of the catarrh or of the inflammatory symptoms accompanying it will considerably reduce, if not abolish, the spasmodic attacks, as the irritation

of the vagus is dependent on the former. The combination of salines with small doses of such sedatives as stramonium, belladonna, and henbane, followed up by iodide of potassium, generally answers well, and these cases are perhaps the most manageable of any.

Again, where asthma is brought about by general causes acting through the blood we have not a pure neurosis to deal with, and our treatment must be directed to the particular morbid state of blood existing, whether it be gouty or syphilitic, or that producing the various skin eruptions. It is in this last form of humoral asthma, as it is called, that arsenic proves of such signal service, especially if the case be at all of a chronic order. It not rarely happens that both the asthma and the skin disease are cured by a steadily pursued course of the drug.

The great majority, however, of cases of spasmodic asthma are purely neurotic, and, like all neuroses, most difficult to treat, the results of drugs on different individuals varying greatly. On one belladonna acts like a charm; on another, by causing dryness of the fauces, it only increases the spasm. Some patients swear by stramonium, others by lobelia; a third class find relief in emetics. There is perhaps no department where empiricism is supposed to hold its sway more absolutely than in the treatment of neurotic asthma; for what between the pills, the papers, the cigarettes, and the pipes, all of which are warranted certain cures, the humble inquirer about the action of drugs may well be perplexed. Though deprecating any attempt at an elaborate classification of the so-called antispasmodics, I would suggest that those of greatest use in the treatment of this kind of asthma may be arranged as follows:—

1. *Stimulant antispasmodics.*—A certain number of asthmatics gain relief by sulphuric ether, by strong coffee, and even alcohol. One of the most recently introduced of this class is nitrite of amyl, which, by acting as a violent cardiac stimulant, sometimes relieves the spasm.

2. *Sedative antispasmodics.*—This is an almost endless class; and if the sufferer perseveres he generally gets relief from one or more members of the group, which includes datura, stramonium, tatula, belladonna, hyoscyamus, tobacco, lobelia, Indian hemp, and others. With regard to stramonium, belladonna, and henbane, their use was dictated, as is well known, by the experiments of Dr. C. J. B. Williams, who found that in animals poisoned by these drugs the bronchial tubes were dilated and incapable of being excited by any stimulus, and presented a marked contrast to the results of opium-poisoning, where the bronchial tubes were found contracted after death. Both stra-

monium and belladonna seem to act most beneficially in the slighter cases; but where they cause dryness of the throat, or even delirium, they seem to have little effect on the bronchial muscle. I found far more satisfactory results follow the use of the extract of stramonium made from the seeds, in quarter-grain to half-grain doses, than in smoking the cigarettes of the leaves, which however have a high repute. Lobelia is a valuable agent when used in sufficiently large doses; but to do much good it must be administered, not in doses of from ten to thirty minims, as is often done, but a drachm at a time, and repeated every three or four hours until some effect is produced. Tobacco when smoked has a sedative influence, but when taken into the stomach is a very unsafe remedy, it being necessary to push it to the extent of producing vomiting before relief comes, and ofttimes alarming symptoms of failure of the circulation to follow. Indian hemp has been used, and with a certain amount of success, but it occasionally gives rise to curious head symptoms, and therefore requires careful watching.

Inhalations of various kinds, and smoking the different cigarettes often do good if the spasm be not very severe; but when this is the case we must expect them to fail, as they generally do, from the great difficulty of introducing them into the system. It is then that hypodermic injections of morphia come in useful, and, by inducing slumber, relax the spasm. The dose should be from one-sixth to one-fourth of a grain, repeated from time to time, and their use is at once contra-indicated by any blueness of the face or signs of obstructed circulation.

In very obstinate cases you will find that sometimes all drugs fail, chiefly on account of the difficulty of introducing them into the system, and the fit gradually wears itself out, generally through the carbonic acid accumulating in the lungs and inducing anæsthesia of the mucous membrane, and thus relaxing the spasm.

I can testify highly to the benefits of chloroform inhalation in the worst cases, in many of which it has acted like a charm, and sent the sufferer into a calm slumber, from which he has awoke free from dyspnœa. In others the relief is temporary, and the spasm returns as severely as ever. The objections to the use of chloroform seem to me to be twofold. First, it sometimes in small quantities—*i.e.*, less than one drachm—causes intermittence of the pulse; second, it cannot with safety be entrusted to the patients themselves.

For these reasons I determined to follow Professor Biermer's example, and try the nearest and safest approach to chloroform—viz., chloral—and I selected for the purpose several cases

where the fits were of long continuance, and marked by only short intermissions. The chloral was given in doses of from fifteen to twenty grains, in an ounce of peppermint water, every three or four hours. The effect in almost every instance was that the patients fell fast asleep after the first dose, and slept in a recumbent posture for a few hours, which they had not been able to do for days and weeks. On awaking there was a tendency of the spasm to return, which was generally obviated by two or three repetitions of the large doses. The breathing gradually became quite free, except when considerable emphysema existed, and here the asthmatic spasm was removed, and only the habitual dyspnœa remained. The doses of chloral were then reduced, and gradually discontinued.

Having succeeded with the most troublesome form of asthma, I next tried the drug in cases where the attacks, though severe, are far more periodic, appearing at a fixed hour every night, and completely disappearing the following day. Here the chloral seemed still more successful, as a large evening dose generally afforded a good night's rest, and steady perseverance with the night draughts seemed to break the habit of the attacks, and eventually to get rid of them altogether. After this I have tried chloral in most forms of spasmodic asthma, and it is not to be wondered at that it has become a favourite prescription in my wards.

In the twenty cases in which I have used it, speedy relief has been obtained in all. In two this has only been temporary; in the other eighteen permanent. In three cases where the scruple doses have been repeated several times, the patients complained of queer sensations in the head, and in one of these, a very obstinate case, where the chloral was continued for days, the patient complained of feeling muddled. Vomiting came on, which disappeared on the appearance of a purpuric eruption on the legs. The drug was of course discontinued, and in a few hours the wheezing, which had ceased, returned. This was the only instance of bad effects following.

I will conclude by strongly urging the claims of chloral to a fair trial at your hands in the treatment of neurotic asthma.

A word on *prognosis*. Patients do not die of asthma, although they sometimes seem on the verge of suffocation. The question of recovery is to a certain extent a question of (1) age; (2) of whether the attacks increase or not in frequency; and (3) what the state of the chest and breathing is in the intervals. If the patient be young—say under fifteen—the chest well formed, the attacks diminishing, and the chest free in the intervals, a most hopeful prognosis can be given. If, on the other hand, the patient be over forty, the attacks

increasing in number and severity, and the breathing more or less short in the intervals, the prognosis is unfavourable.—*Lancet*, *Oct.* 11, 1873, *p.* 515.

35.—ON THE USE OF ERGOT OF RYE IN THE HEMOPTYSIS OF PHTHISIS.

By Dr. FRANCIS E. ANSTIE, Senior Assistant Physician to Westminster Hospital, &c.

I very early passed from the stage of implicit belief in styptics to a total distrust of them (as internally administered); but for some years past I have seen reason to think that their power is real, though far more limited than many persons have supposed it to be. That the action of the more commonly employed internal hemostatics is to the last degree uncertain (even though in a moderate number of cases they may apparently act with great promptitude and effect), I think no one of large experience will deny, unless he has entered upon the inquiry with the determination to see none but successful results. With regard to some remedies which (known to be *locally* more or less styptic) are continually given internally for hemorrhage, by the merest routine, he will quickly make up his mind. He will conclude that, for certain, the mineral acids, in any practicable internal doses, are not directly hemostatic at all, and are not in the least reliable when the momentary object is to stop the bleeding. And probably he will in the end conclude that, putting aside the case of digitalis, which is peculiar, there remain, among the ordinarily reputed remedies for hemoptysis, only (1) gallic acid, (2) acetate of lead, (3) either of these combined with opium, (4) alum, (5) turpentine, and (6) sesquichloride of iron, which yield anything like decisive results, while each of them in turn is very apt to fail. Certainly he would conclude, without the least hesitation, that we are ignorant of any but the vaguest clues to the discriminative use of internal styptics; yet there must be serious differences in the applicability of different forms of hemorrhage. To take a very striking example, what do we know of the reason for the difference between the magical styptic action of turpentine in hemophilia, and even in purely symptomatic purpura, and its capricious (though occasionally good) results in ordinary phthisical hemoptysis?

On the other hand, there is, in the case of ergot, good *à priori* ground for supposing that it would prove to be a more universally applicable remedy in hemoptysis than any other that has been named. Proofs of its general physiological action have accumulated on all sides of late years, and we know

with certainty that, besides its familiar action on the uterine walls, it contracts the smaller arteries, and slows the heart. In these respects it would seem likely to check pulmonary hemorrhage in the same way as digitalis does, though probably with more vigour, in both these directions, than would be shown by any safe and practicable doses of digitalis. This leads me to those special remarks on digitalis as a pulmonary hemostatic which I had reserved till now. I think that any person who has patiently tried digitalis, both in ordinary hemoptysis and also in pneumonia according to the German system, will readily acknowledge its very decided power of diminishing the blood in the sputa in both diseases; and I believe that a careful observer would convince himself that this occurs in direct proportion to the degree in which the heart's action is slowed, *provided it continues regular*. But whatever may be the ultimate conclusion as to the working of digitalis in pneumonia, it certainly is an inconvenient agent to rely upon in the suppression of hemoptysis, for though very powerful *ad hoc* it produces in an inconveniently large number of cases a temporary fluctuation in the force and regularity of the heart's pulsations. This is very disquieting, and sometimes alarming to the patient, and is likely to more than neutralise any direct benefits from the drug. Now, with regard to ergot, I can affirm with great confidence that there is nothing to be apprehended. I commenced the use of this drug, for a variety of purposes, several years ago, and have probably given it as largely and continuously as anyone has done. Ever since 1868 (on the suggestion of Mr. Woakes) I have been in the habit of giving ergot in neuralgia and other nervous complaints, and in order to test the range of its powers have often administered it in large doses for many weeks together. Even the continuance of forty-minim doses thrice daily for six weeks or two months has never produced any untoward symptom beyond a general nervous depression, not greater than that which often follows a long course of large doses of bromide of potassium. I need not say that any employment of ergot for hemoptysis would involve only much briefer periods than this: for unless it pretty quickly produced a decided impression we should have no reason to expect good from its continuance; and after hemorrhage has ceased it is probably only necessary to continue the ergot, in diminishing doses, for a few days longer.

I think we have now established the facts (*a*) of the direct action of ergot in the cases which I have recorded; (*b*) of its superiority in several of these cases to other styptics that had been tried; (*c*) the probability, from physiological analogies, that ergot would act more universally as a checker of hemop-

tysis than the routine remedies with which we are familiar; (*d*) also that it is perfectly safe for the purpose in view, and in this respect is superior to digitalis, which otherwise resembles it a good deal. Some further illustrations of its special activity should, however, be given; and I quote, in the first place, from my own cases. Case II. in series *C* (advanced phthisis with rapid tissue destruction) was one of the most ugly and threatening cases of hemoptysis that I ever saw; in addition to the formidable symptoms already mentioned, there was a history of an epistaxis, some twelve years before, which proved alarmingly uncontrollable, and very nearly killed the patient; the fact that such an immediate impression was produced by the ergot in this case has a great significance. Case I. of series *B* is also very convincing; the alcoholic type of phthisis, when it follows the course described, is a disease in which hemorrhage, if it occurs at all, is often very violent and intractable, and here it was plain that the direct action of the ergot upon the heart and upon the small arteries was both prompt and efficacious.

One of the best proofs of the reality of a supposed action of any drug is afforded when the effects, which were comparatively slight or dubious after the administration of the rougher preparations, became more marked and apparent in proportion as fewer and more concentrated preparations come to be employed in practice. Now this is precisely the history of ergot as a remedy for hemorrhage. Used at first in the form of infusion or tincture, it was only moderately efficacious; and, indeed, so late as 1870, Nothnagel expressly states that which I have been endeavouring to show can no longer be considered true, viz., that ergot is not superior to acetate of lead, or sesquichloride of iron, in hemoptysis. But the liquid extract, given by the stomach, has given even better and more certain results, not only in my hands but in those of several others. And the hypodermic injection of Bonjean's ergotin (which I have used for other purposes, but not for hemoptysis) has given still more excellent promise, more especially in the hands of Drasche, of Jamieson, and of Dr. Currie Ritchie, of Manchester; the latter gentleman published an excellent paper on the subject in the Practitioner for December 1871.

Meantime there have sprung up other collateral illustrations of the effectiveness of ergot in producing contraction of the arterioles and moderating the heart's action, of which the most remarkable is Langenbeck's discovery of the therapeutic use of ergotine injections in aneurism. It is true that this latter subject is as yet not at all completely developed; but enough has been proved to strengthen materially the general mass of evidence in favour of the belief that ergot acts precisely in the

manner in which we should desire a pulmonary hæmostatic to act. I of course do not say that we shall not possibly discover a yet more useful remedy for hemoptysis, but at present we probably possess none equal to it. Over and above the important resource which ergot seems to afford when hemorrhage is actually dangerous in amount, and when such remedies as gallic acid, acetate of lead, or muriate of iron have failed, I think that ergot is likely to be the most appropriate of all hemostatics for the hemorrhage of early phthisis, where it may be hoped that the lung has as yet undergone very little structural change. The prompt suppression of all oozing into the air-cells which, in view of the danger of phthisis *ab hæmoptoe*, it is so important to effect, can probably be in no way so effectually accomplished as by the employment of ergot.

Nor do I prefer the gastric administration, although in the cases recorded I employed it. For getting the best results I can scarcely doubt that the hypodermic injection of ergotine is a decidedly superior method. But I was personally anxious to see whether the remedy would prove practically effective when given in a form which is much less troublesome, and consequently more adapted to every-day practice, than the hypodermic injection would be. I hope I have shown that it is effective in a very encouraging degree, and that the profession will soon come to recognise the preferability of a direct recourse to its use in hemoptysis, instead of the routine employment of a series of astringent remedies which are considerably less uniform in their action.—*Practitioner, May* 1873, *p.* 276.

36.—ON THE INFLUENCE OF DISTENSION OF THE ABDOMEN ON THE FUNCTIONS OF THE HEART AND LUNGS.

By Dr. FRANCIS SIBSON, F.R.S., Consulting Physician to St. Mary's Hospital.

[In treating this subject, Dr. Sibson passes rapidly in review the medicines most frequently serviceable, such as the warm and cordial tinctures, camphor, turpentine, assafœtida, and creasote. Charcoal is of little or no use in these cases. His object is rather to bring before the reader the non-medicinal means that may be employed for the relief of the distension.]

This gas, as I have said, is secreted, not by the structure of the stomach, but by the food. If you cut off the supply of food, sooner or later, when that already in the stomach has passed away, relief will be obtained. But we cannot cut off the supply of food; therefore we must select such kinds of

food as give the greatest amount of nourishment, and tend to set free the least amount of gas; for, although every kind of food will produce gas when the powers of digestion are weak, yet some kinds will do it much more freely than others. My rule is simply this: Take no bread, but eat biscuits instead. Take no solid vegetables, but use lemon-juice freely. Squeeze some of it over all your food. Shun soup, cucumbers, and fruit. Prefer meat, fish, fowl or game, tender, but perfectly fresh, lightly cooked and juicy—not hard and dry; or take it thoroughly soft and fragrant, as sent up by the hand of the artist. Take no rich sauces, or indeed, any other sauce, than wholesome fresh lemon-juice or mushroom ketchup; no twice cooked, or salted, or seasoned, or dried meat. Let everything be fresh and savoury. Under this simple rule of substituting biscuit for bread and fresh lemon-juice for vegetables, relief will often be obtained from distressing symptoms indicating danger. Let the food be agreeable to the eye, fragrant to the nostril, and savoury to the palate; and, invited by those senses, the influences of the nervous system will bring those juices into the mouth and stomach that are required for the digestion of the food—juices that are attracted if the food is attractive, repelled if the food is repulsive.

The rules, however, that I have given are but a portion of the proper treatment. Each case stands upon its own grounds, and must be treated according to its own demands. In treating each case, the laws of nature must be strictly enforced. Departure from those laws has brought on the illness; and in a renewed obedience to those laws, we shall usually find the best road to restoration of health. The right treatment of disease is neither expectant nor heroic, but is a treatment founded on knowledge, a watchful interest in the patient, moderation, and good sense.

One patient had been quite well until the previous April, when, one night, after a late supper, he woke up fighting for breath and life. He raised a thin and frothy phlegm, which was stained the next day with blood. He had suffered from no such attack for a month; but he could not lie down; he was short of breath, and felt, as he said, inflated—blown out—sometimes almost ready to burst. His heart was very large, its action being powerful. There was a doubling of the first sound over the ventricles, and feeble first sound, and loud second sound over the aorta. Effusion was present in the right side of the chest. There was a mere trace of albumen in the urine, but I found in it one large waxy cast. The kidneys were the true seat of the disease. Under the influence of the restricted diet I have described, and the general treatment, he lost the distension of the stomach; he could lie down on one

pillow; he had no return of the sense of suffocation; and the fluid in the right side of the chest disappeared. The condition of the heart and that of the kidneys were not materially altered.

Here is another case of a patient (represented in this little diagram) who complained of "trouble" over the region of the heart. His abdomen was swollen; the stomach displaced the lower boundary of the heart upwards, so that it was situated in the fourth space. Subsequently, under a restricted diet, the upper boundary of the stomach and the lower boundary of the heart fell almost to the extent of an inch. The heart having gained an increase of space, lost its "trouble;" and, though he was ill in other respects, the distension of the stomach disappeared.

Another diagram that I have here shows a marked difference in the position of the heart during the second, as compared with the first, observation made eleven days previously. This patient complained that he had pulsation through the body, and that the heart stopped for two or three beats. This state was worse after dinner, and was accompanied by fulness in the head. The irregular action of the heart disappeared with the descent of that organ that followed upon the diminution of the size of the stomach.

The last case to which I shall refer gave me at first some anxiety from its obscurity. This patient was a fine-looking man, with a large but not overgrown frame. He was of good colour and complexion. He complained of great shortness of breath, and he had a dropsical swelling of the right lower limb. The only unhealthy condition that I could discover in his internal organs was a distension of the stomach from gas. I will allow that I felt as if there must be some other cause, but I could find none. He took a dry diet; and although, a week later, the stomach was less distended, yet his right leg was still swollen, and his breathing short. He persevered, and gradually lost the shortness of breath and the swelling in the right leg, and he regained his usual good health.

Distension of the colon is illustrated in these diagrams. You will observe that, in these instances, the transverse colon is no longer transverse. In these they all form a loop from above downwards, the tip of the loop being midway between the navel and the pubes. You will also see that the ascending colon increases greatly in length, starting from its head in the crest of the ilium. You may remark, on looking at these two diagrams, that inasmuch as the top of the ascending colon immediately abuts upon the lower surface of the liver, the elongation of the ascending colon directly elevates the liver. So, also, with regard to this double flexure of the transverse

colon. In one instance, you see the loop is single, and in another the loop is double. You will see that the descending colon is not in any of these cases, except perhaps in one very greatly distended; yet there is in all of them great distension. In this diagram, representing a back view, the descending colon is distended quite as much as the ascending and transverse are in any of the others. In another instance, you see that the liver on the right side, and the stomach and spleen on the left, are both lifted up, as it were, upon the top of the lengthened colon. You may remark in these cases that the liver rises very high; in one instance, quite into the lower end of the fourth space; and in another, as high as the third space. And you may observe how much higher the upper boundary of the right wing of the diaphragm, which is lifted up by the liver, is than the upper boundary of the left wing of the diaphragm, which is lifted up by the stomach. The result is that, in these cases we have the heart pushed over to the left side, owing to the elevation of the liver, which tends to displace the heart to the left; and while the right lung is compressed from below upwards, the left lung is encroached upon by the heart, which is pushed aside by the liver.

You will at once see, from the study of these diagrams, how seriously distension of the colon must affect the organs of the chest, not only the lungs, but also the heart, by displacing that organ to the left; whereas, in the case of the stomach, its displacement tended rather to the right.

The next set of cases that I shall bring before you are those in which the stomach, the colon, and the small intestines are all distended. In this diagram, distension of the stomach is not extreme, but there is extreme distension of the ascending, the transverse, and, I believe, the descending colon, and extreme distension of the whole of the small intestines owing to peritonitis. You will observe that this diagram shows, better than any other, the oval shape that the distended abdomen assumes. Contrast it with this one, which shows the shape of health, like a reversed sugar-loaf rounded at the top, while the other is a true type of complete abdominal distension, being a perfect oval. In this other case, where the abdomen is perfectly flaccid, you have neither the reversed sugar-cane shape of health, nor the oval of distension, but a double concavity. The centre of the abdomen is narrower than either the top or the lower part. We see here, in another diagram, the effect of immense distension upon the walls of the chest. See how these cartilages are pushed aside to the left, and those to the right, so that there is an addition of nearly one-third to the width from side to side. Again, you may perceive that the cage of the chest is lifted up by this great oval mass below pressing

upon it from below, and this will account for the remarkable fact that, although we have the heart and lungs so elevated by the diaphragm thus pushed up, yet the lower boundary of the heart is very little above the lower boundary of the sternum; the lower boundary of the lungs is very little above the position in which we often find it in a perfectly healthy body. If you look at the aorta you will see how completely that artery has been driven over to the right side. If you compare the breadth of the heart from side to side, with the length of the heart and the great vessels from below upwards, you will see that the whole organ is, in point of fact, compressed upwards. In health, as you may see in several of these instances, the apex of the heart is lower than the lower boundary of the heart behind the lower end of the sternum. In this instance, the apex of the heart has been lifted upwards and is a third of an inch higher than the lower boundary of the heart at the end of the sternum.

We find in these cases—that the heart and the lungs are put to an immense disadvantage—that the lungs are compressed and the reserved air driven out of them—that the heart is compressed and the blood driven out of it—that the ventricles are diminished in size and are incapable of receiving so much blood as they did before—that there is an obstacle to their action, and that they are incapable of sending out the blood that they have already received with the ease that they did before. Again, if you look at the walls of the chest and the diaphragm, you see what an obstacle is put to the very limbs, by which, as it were, the lungs expand and the heart is moved. The cartilages and ribs are pushed aside and lifted upwards. The cage of the chest has reached the limit of its powers by the elevation given to it, and permanently given to it by the distension of the abdomen. Again, look at the diaphragm, lifted up by this immense distension. How can it act downwards? If the diaphragm is affected with peritonitis, the inflammation paralyses the inflamed muscle. The whole contest is then narrowed to the smallest bounds; and the whole work of respiration must be done by the few ribs remaining unassailed at the top of the chest. In cases of peritonitis, respiration is what is very well called high. The whole of the breathing is performed by the laboured lifting up of the upper cartilages, the upper ribs and the clavicles. All the muscles that act—the sterno-cleido, the scaleni, the levators of the ribs and the scapulæ, are hard at work, and, in spite of that, you see visibly in the face that the patient is undergoing chronic asphyxia, and must soon die unless his breathing is relieved.

Here is another case: a repetition of the other, but a little less extreme. It shows in what I would call a rather beautiful

and orderly manner, a series of tiers, one above another, the top one made by the distended stomach, the second by the circular curve of the transverse colon, and the third by the small intestines. Here is also another case, showing tier above tier in the same way, the intestines below, the transverse colon in the middle, the stomach above, and above that the heart. Here is also another, shewing the intestines below, the transverse colon at the middle and the stomach at the top, all pushing up the heart and lungs and interfering with their work. I could show to you other cases; but these are sufficient.

The tendency of this great distension of the stomach and intestines is to push the heart directly upwards, to flatten the heart from below, and to displace it somewhat to the right side. You see in this diagram that the aorta, instead of being shielded by the sternum, lies almost entirely to the right of that bone. The pulmonary artery, on the contrary, instead of lying entirely behind the spaces, is almost entirely behind the sternum. A similar condition obtains in this other case. You see that the apex has been raised. It is nearly half an inch higher than the lower boundary of the centre of the heart, although, naturally, it ought to be half an inch lower than that lower boundary. The position of the apex is the test of the extent to which the heart has been compressed upwards.

There are two means by which the treatment of great distension of the stomach may be met, independently of drugs; and I would say to you that when distension is extreme, drugs are worse than useless. They add, by their offensiveness, to the misery of the patient. These two modes are the insertion of the œsophageal tube into the stomach through the mouth, and the insertion into the stomach of the fine point for injecting morphia under the skin. I have the particulars of a case that occurred in this hospital—that of a man who had the most serious threatening of death in consequence of great distension of the stomach. The circumference of his chest was thirty-nine inches. We introduced the œsophageal tube, and a large quantity of gas escaped. The circumference of the chest was reduced from thirty-nine inches to thirty-seven, and for a time the man experienced perfect relief. Before this was done, he had great distress; after it, he was at ease. The relief lasted for some days. He had kidney-disease and various other serious affections; and after a time he died.

Another case, that I saw with the late Dr. Williams of Swansea, was more fortunate. The patient had acute rheumatism of a low type. The stomach was extremely distended, and respiration was interfered with in consequence. Dr. Williams introduced the tube; a large quantity of gas escaped; the powers of respiration were restored, and gradually the

patient recovered completely, lived for many years afterwards, and is, I believe, now alive. With regard to the other method —the insertion into the stomach of the fine point for injecting morphia under the skin—I have never had the opportunity of employing it; but I have been always ready to use it if required; and I believe, though I cannot put my hand on a case, that it has already been practised with advantage and success.—*British Medical Journal, Aug.* 3, 1873, *p.* 105.

37.—ON ARTIFICIAL RESPIRATION AFTER THE OPERATION OF TRACHEOTOMY.

By Dr. BENJAMIN W. RICHARDSON, F.R.S.

At the conclusion of a lecture published in the Medical Times and Gazette on Dec. 4, 1869, in which was described the double-acting elastic hand-bellows I have invented for artificial respiration, I took occasion to point out that in various cases of acute disease—such as cases of bronchial spasm or of failure of respiratory power,—when death is at hand, the practice of artificial respiration, by the bellows, might prove of service. The following clinical history, briefly told, illustrates forcibly the truth of this proposition:—

A child, aged 4 years and 4 months, was under the care of my friend and neighbour Dr. Neale. She had shown for ten days some obscure symptoms, varying in intensity, of irritation of the throat, with intermittent pyrexia. On Sunday, the 20th of last month (July), on his morning visit, Dr. Neale found the child breathing heavily, and again feverish, On examining the throat, he discovered a diphtheritic patch on each tonsil; he detected also a croup-like respiration. A little later in the day I was called to meet Dr. Neale in consultation. We agreed as to diagnosis and treatment, and all progressed well until Tuesday morning, the 22nd, at three o'clock. At that time the symptoms changed, food and medicine were refused, and in a few hours the respiration was embarrassed from obstruction in the larynx. Severe paroxysms of spasmodic breathing also occurred at frequent intervals. An emetic gave great temporary relief; but the symptoms afterwards increased, and at 2 p.m. were so urgent that the question of the performance of tracheotomy came under consideration. At half-past six, at a consultation between Dr. Neale, Mr. Christopher Heath, and myself, the operation was determined upon as a necessity. I administered methylene bichloride, which acted promptly and effictively, and Mr. Heath operated.

Recovery from the anæsthesia was rapid, and the little patient soon began to breathe freely through the tracheal tube.

Dr. Neale having to leave, I undertook to remain until his return. About an hour after the operation the child was suddenly seized with an attack of severe spasmodic breathing, followed by the escape of tenacious frothy mucus through the tracheal tube. I cleaned the tube freely, and drew up the mucus through the tube, but the paroxysm continued; the features became livid; there was a sharp convulsion; the natural respiration ceased; the pulse ceased; and, to all appearance, there was death. At this moment, affixing an inner tracheal tube to the double-acting bellows, I thrust the inner into the outer tracheal tube, and commenced steadily to keep up artificial respiration. I confess I had no great expectation of useful results; but to my relief, after sustaining the artificial respiration for a period of three minutes, the patient recommenced to breathe independently, and by the time Dr. Neale, who had been sent for, returned—about ten minutes,—she was quite conscious, warm, and breathing freely. We watched alternately through the night, and the night was passed comfortably. Food was freely taken, sleep was natural, the pulse was firm and regular, never exceeding 120, while the respiration, though at times quick (36 per minute), was not laboured. At 8 a.m. on Wednesday, the 23rd, Mr. Lovell and Mr. J. E. Neale being now in attendance, a paroxysm of very severe dyspnœa and spasm recurred, but was quickly relieved by the use of the bellows, and on Dr. Neale's arrival, later in the day, the recovery was so complete that the child was playing at "catch ball" with a sponge. She progressed favourably until 8 p.m. of this day, the 23rd, when from a sleep of about twenty minutes she awoke with another violent paroxysm of the dyspnœa. *In extremis*, Dr. Neale again set up the artificial respiration, and in two minutes the natural respiration was restored. After this there was natural action of the kidneys, and milk was freely taken. At 9 p.m. another extreme attack of the dyspnœa supervened, relieved in the same manner after the use of artificial respiration for five minutes.

At 9.30 I rejoined Dr. Neale, and we found now, for the first time, moist crepitation from exudation or condensation of fluid in the upper surfaces of both lungs. Relief in removal of some of this fluid was afforded by a device of Dr. Neale's of making a Higgins syringe act as an aspirator, and there followed a period of gentle and hopeful breathing. But at 10.30, on the occurrence of another of the extreme paroxysms of dyspnœa, the respiration ceased altogether, the pulse ceased, the countenance assumed the lividity of death, and death, indeed, once more appeared complete. The tracheal tube was removed, the tube of the bellows was inserted into the tracheal opening, artificial respiration was established, and after an interval, as

far as we could measure, of five to six minutes, the natural respiration was restored.

From this time, unhappily, the pulmonary lesion increased; the obstruction to the breathing passed from a local to a general condition; the bronchial passages became closed from accumulation of fluid; and absolute death, without return of asphyxia and without a struggle, occurred at 1.30 of the 24th.

To have to record the ultimate fact of death in this case, after such restoration from apparent death, is indeed a painful duty. Yet the one particular process for restoration—artificial respiration—is thereby, in no degree, lessened of its value. It gave a renewal of thirty hours of life, during which if we had known more respecting the removal of those fatal conditions which so commonly commence in the extreme or peripheral parts of the pulmonary organs, we might have made the temporary safety complete and permanent. Step by step, and with that agony of experience which ever attends the highest efforts of the human mind, we shall attain one day even to this perfection. We shall acquire a still more perfect method of maintaining artificial respiration; we shall learn to introduce, with good effect, other gases and vapours with the common air; we shall regulate pressure on the pulmonary surface; we shall sustain, by some simple means, the muscular force; and we shall supply to muscle the stimulus it has lost from the exhausted nerve. In a word, we shall hold mastery of conditions in the presence of which the sick person cannot die. This, the *summum bonum* of medical science and art, may be afar off; but he is no physician who does not live for it, and the history I have written above demonstrates that one of the most important steps towards it is laid for our advancement.—*Medical Times and Gazette, August* 2, 1873, *p.* 117.

DISEASES OF THE ORGANS OF DIGESTION.

38.—ASCITES IN INDIAN PRACTICE—USE OF THE DRAINAGE-TUBE.

By Dr. JAMES DONALDSON, late of H.M. Madras Medical Staff.

[The habit of body of the native Hindoo is quite different from that of the inflammatory European. The former easily succumbs to sudden accessions of violent diseased action, but is all enduring as to chronic forms of illness, and tolerates an amount of handling of vital organs and interference with functional processes which would scarcely be believed by any one

unacquainted with the peculiarly phlegmatic temperament of the natives of India. The following case is related by Dr. Donaldson in illustration.]

Disease, Ascites.—Ponamah, female, age 35. Caste or country—Hindoo, native of Madras. Admitted into the Government hospital at Vepery, second district of the presidency town of Madras, on 30th April, 1864, in a distressing state of helplessness, breathlessness, and weakness, enormously swollen, abdomen distended apparently to fullest limit, skin tense. As it is not intended, and would serve no useful purpose in these succinct histories, to give *minute* details of symptoms and treatment here, it may be merely noted that the object primarily looked to in this instance was, bringing the patient into the best state of preparation for the operation of paracentesis abdominis, with the intention of performing it at the earliest date. Mild aperient medicine, warm baths, and saline diuretics were the remedies indicated. There was no history detailed to account for this abnormal collection, no apparent disease of liver, heart, kidneys, or other organ; the urine not noted as being much different from the ordinary standard unless small in quantity, which was to be expected, and which would naturally be more prominent from the high temperature prevailing at that time, viz., 95° to 100° Fahr., in the large, well-appointed hospital, daily. On May 6, there appearing to be no reason why the operation should be delayed, and the patient being exceedingly anxious for its performance, "tapping" was carried out in the usual way, the patient lying well over on her side, and twenty-two (22) pints of fluid drawn off. (Patient would weigh, in health, about 90 to 95 lbs., but this could not be very exactly ascertained.) The relief afforded by the removal from the abdominal cavity of above 400 ounces of fluid was very grateful, and a bandage being applied with comfortable pressure, the patient expressed herself as feeling "restored to health." On careful examination, now that the different organs were more within reach of proper observation, no chronic disease of liver, heart, or kidneys was discovered. The heart's action was feeble, nothing more. The patient, with good nourishment, and very moderate and temporary use of stimulants, soon felt strong; but the process of re-filling commenced but too soon, and with sufficient quickness. As diuretics, the acetate, nitrate, and bicarbonate of potass were given, with iodide of potassium; also carbonate of ammonia and squill; the old "compound iodine ointment" was rubbed assiduously into the abdomen, and careful bandaging kept up; but in the end of May the distress and dyspnœa were again such as to cause the poor woman to beg for relief. On 2nd of

June tapping was again had recourse to, nearly eleven pints of fluid on this occasion being removed. Now, as this probable often repeated paracentesis seemed to promise only very temporary relief, with the almost certain prospect of rapid prostration of patient's strength, the idea was entertained of not only withdrawing as much fluid as possible, but farther, of allowing it to drain away as it formed, with the hope that by aid of steady, carefully-applied pressure kept up, even if a moderate amount of inflammation should be excited in the cavity, a radical cure might be expected. This view of the case, and this proposition being placed before the patient, she readily assented, preferring any risk (as natives of India usually do) to the prospect of repeated accumulations and operations. Accordingly, after as much of the fluid as possible had been drained off, the bandage being carefully adjusted, the cannula was left in, and tied in such a way as to act like a drainage tube—a small cork having been first fitted to it, so that, instead of the drain being constant, the cork could be taken out at short intervals, and any little collection of fluid allowed to run away. On the 3rd June this is the report,—"Little or no fluid has passed by the cannula since the operation; there is a little pain just where the cannula is inserted; no general pain, no threatening of peritonitis, no fever, no bad symptom, but the patient, as on the former occasion after operation, is weak, and the pulse is feeble. Bowels and kidneys acting well." On the 6th, little fluid appeared to have been secreted, and the report is,—"Abdomen getting smaller instead of immediately commencing to increase in bulk as it did last time, and the flannel binder was tightened yesterday; no pain or uneasiness of any kind; patient feels well and is sitting up." On the 16th, the cannula had got dislodged. It was not thought advisable to try to replace it, and report is,—"Water threatening to collect again, but not much; the compound iodine ointment to be rubbed in, and the diuretic medicines given as before." 21st June,—"Feels easy and well, little change of any kind; to continue idem." On the 15th July, patient was well in all respects (except that the fluid in abdomen had increased somewhat) and afraid that it would augment farther, and bring on the old, uncomfortable symptoms, she wished the cannula replaced, and the drain re-established; but this was not considered advisable or necessary. After this date there was occurrence of fresh symptoms—viz., congestion in several organs (liver, lungs, and kidneys), with pain in the right hypochondrium, some oppression of breathing, and albuminous urine; there was no retarded circulation from re-accumulation of water to account for this, and probably the patient had got a chill from exposure to the strong hot land wind, or

the stronger chilling sea breeze which daily succeeds it, after the free perspiration caused by the excessive heat. Besides warm baths, and re-induction of the skin's action, all that was thought necessary was the application of a large blister over the abdomen, just below, and over the lower portion of the liver. These means were effectual. On the 21st July "patient was easier, breathing comfortable, lungs relieved, pain gone, urine copious, and free or nearly so of albumen;" the medicines ordered to be continued, the blister dressed with mild ointment. On 29th, "collection of fluid apparently stationary, not increasing." From this date the patient's condition, from being "stationary," began to improve; the fluid in abdomen, instead of increasing, decreased; and the report is,—"From this date the poor woman slowly but steadily improved in health, the various organs re-assumed their natural state, and satisfactorily performed their functions. The remaining fluid in the abdomen was absorbed, the belly regained its proper shape, and she was discharged from hospital, after being kept sufficiently long under observation, apparently 'quite well" in the beginning of January following."

There were, I think, several points of interest in this patient's illness—*e.g.*, the large accumulation of fluid in this small woman without apparent organic disease; without apparent noteworthy obstruction to the return of blood by the portal vessels to the heart; without any previous history of active inflammation of the peritoneum, or obvious chronic inflammation of the same, &c. Sir Thomas Watson records a case almost identically the same in its more obvious aspects, referring to it as "a condition he believes he had met with." Again, the rapid re-filling of the sac without any manifest exciting cause after the first operation of withdrawal, under nearly the same existing conditions; the circulation being so much more free by the abstraction of the heavy mass of fluid before weighing on the abdominal organs, and oppressing, almost paralyzing, the action of the diaphragm. But the main point was the establishing a drain from the peritoneal sac with the strong risk of almost inviting, as it were, inflammation within the cavity. I cannot call to remembrance seeing such procedure anywhere recommended, and I would not have proposed such in an European, nor ventured upon it in private practice, but "the end justified the means," I hope. I had only one other very similar case in India, the details of which need not be given, except just stating the leading facts. The patient was also a female, native of the Madras Presidency, admitted into the Garrison Hospital, at Vizianagram, in 1860:—had been "tapped" (by my predecessor in office), the sac had gradually refilled; there was no discovered organic disease, and no

"history." I operated once, and drew off a large quantity of fluid, then (on the second occasion) left the canula tied in, and established a drain. There was no bad result; on the contrary, the process of re-filling was checked, the drain was free, and I was indulging hopes of a complete cure when the woman, tired of hospital, or thinking she was well enough, went away without "gratia," seu "vale," as is very common with the natives of India when the spirit moves them, and I saw her no more; could not even hear what had become of her during my short remaining term of duty in the station.—*Glasgow Medical Journal, May* 1873, *p.* 306.

DISEASES OF THE URINARY ORGANS.

39.—ATROPHY AND SUPPURATIVE INFLAMMATION OF THE KIDNEY FROM RETENTION OF URINE.

By Dr. GEORGE JOHNSON, F.R.S., Physician to King's College Hospital, London.

The effect upon the kidney of retention of urine varies according to the nature and seat of the impediment. It differs, too, according as the obstruction occurs gradually or suddenly. One of the most frequent causes of renal disease consequent upon retention of urine is stricture of the urethra. The urinary organs behind the stricture undergo changes of structure in proportion to the degree and the duration of the obstruction. The canal of the urethra on the vesical side of the stricture becomes dilated; its mucous membrane is frequently inflamed, and secretes pus. The muscular coats of the bladder become thickened by hypertrophy, and its mucous membrane often inflamed and sacculated. The obstruction then affects the ureters, one or both of which may have their canals dilated and their walls thickened; and at length the natural cavities of the kidney—the pelvis, infundibula, and calyces—undergo the same process of dilatation. The medullary cones become flattened out by the pressure of the retained urine. The cortical substance of the gland is expanded, and presents lobed bulgings on its surface, which correspond with the original lobes of the embryo kidney. The glandular tissue is squeezed between the distended interior cavity and the fibrous investing capsule; and the intertubular capillaries are compressed by the dilated tubes. Thus the circulation is impeded, and the result is atrophy of the gland, which may by degrees be converted into a membranous cyst, all traces of glandular structure being lost.

It is but seldom that the kidney undergoes much dilatation without the occurrence of other structural changes. The mucous membrane of the dilated pelvis often presents irregular inflamed patches, and secretes a purulent liquid; and the apices of the medullary cones are frequently ulcerated. Then, as the mischief extends, inflammatory deposits occur in the substance of the kidney, and numerous small abscesses are scattered through the cortex. One or more of the abscesses on the surface may burst through the capsule, and then the kidney may be found imbedded in pus.

When the retention of urine is the result of stricture, or of enlarged prostate, or of calculus with thickening of the walls of the bladder, or of atony of the muscular coats, both kidneys are usually affected simultaneously, but in different degrees; but when one ureter is obstructed by a calculus, or by a cancerous growth, the structural changes are limited to the corresponding kidney.

The explanation of these changes is not difficult. The secreted urine is unable to escape in consequence of the obstruction in front; there is, therefore, an accumulation, first in the ureter and pelvis of the kidney, and later within the uriniferous tubes. The tubes become distended by the retention of their own secretion, just as some tubes in the small red granular kidney, having lost their lining of gland-cells, but continuing to secrete a serous liquid, become distended and dilated into cysts. The epithelial lining of the straight tubes is disintegrated and destroyed by the pressure of the retained urine; and at length some of the tubes in the cortex, whose basement-membrane is more delicate than that of the cones, give way, and allow their contents to become infiltrated amongst the intertubular capillaries. The infiltration of acid urine may cause the immediate formation of coagula within the capillaries, and, as a consequence of localised capillary and venous obstruction, irregular atrophic puckerings of the gland occur, somewhat similar to those which result from capillary embolism, to which I have before referred. But the escape of urine through the broken walls of the tubes may excite suppurative inflammation and abscess. A rapid cell-formation takes place between the tubes; and soon the glandular structure is disintegrated, and replaced by inflammatory products. The changes within and between the uriniferous tubes are a miniature representation of what happens on a larger scale when a distended urethra gives way behind a stricture, and a perineal abscess results from the infiltration of urine into the submucous tissues. Bear in mind that the changes within the substance of the kidney are due to the retention and accumulation of the newly secreted acid urine within the tubes, and

not to the regurgitation of fœtid ammoniacal urine, as has recently been suggested. Without doubt, the urine in the bladder in cases of old stricture, vesical calculus, and cystitis, is often fœtid from decomposition; but the regurgitation of such urine into the uriniferous tubes is a physical impossibility. If you have ever attempted to inject the tubes from the pelvis of the kidney, you will have found the task a very difficult one, in consequence of the resistance offered by the liquid and solid contents of the closed tubes. Obviously, then, during life, while streams of secreted urine are perpetually flowing through the tubes, it is impossible that urine from without can regurgitate into them. The tubes are dilated, and some of them ultimately ruptured, by the retention and accumulation of their contents, and not by the regurgitation of urine from the pelvis of the kidney. Then intertubular coagula and suppuration result, from the infiltration of urine escaping from the torn tubes amongst the intertubular capillaries and veins.

The *Symptoms* of renal disease consequent upon an impeded escape of urine are usually more or less marked by the diseased condition of other parts of the urinary organs. The mucous membrane of the bladder in cases of stricture, vesical calculus, or enlarged prostate, usually secretes pus; and there are no means by which this can be distinguished from matter derived from a suppurating kidney. For the suppurative process in the kidney rapidly destroys the tubular structure of the organ; the pus, therefore, is not moulded within the tubes, and there is no microscopic evidence of the renal origin of the pus. Chemistry, again, affords no more assistance than the microscope. The urine, which contains pus, is always albuminous. The coagulability of the urine by heat and acid is, therefore, no indication that the kidneys are implicated, except when the degree of coagulability is out of proportion to the amount of pus, and it may be blood, mingled with the urine. Chemical analysis affords little practical aid in estimating the efficiency of the kidney. The urine is usually fœtid and alkaline, and much of the urea is decomposed into carbonate of ammonia while the urine is still in the bladder. A low specific gravity of the urine with a scanty secretion would be a suspicious condition, and especially so when associated with indications of uræmia, such as drowsiness, headache, vomiting, and a brown dry tongue, with an excess of urea in the blood. Pain and tenderness in the region of one or both kidneys may be severe when, with sudden retention of urine, there is great distension of the cavity of the kidney; but, in cases of long continued and slowly increasing obstruction, these symptoms bear no proportion to the amount of structural change in the kidneys;

and it sometimes happens that the first indication of serious renal disease is afforded by the occurrence of alarming symptoms of uræmic poisoning, quickly passing on to fatal typhoid collapse and coma, with a low temperature. The cases are few in which the kidneys are sufficiently enlarged by distension and expansion to form a palpable tumour in the lumbar region. *British Medical Journal, May* 31, 1873, *p.* 605.

40.—ON ACUTE CYSTITIS SIMULATING ACUTE BRIGHT'S DISEASE.

By Dr. GEORGE JOHNSON, F.R.S., Physician to King's College Hospital.

It has happened to me to meet with a considerable number of cases of acute inflammation of the mucous membrane of the bladder, unconnected with stone, stricture, or gonorrhœa, which, in consequence of the urine being blood-tinged and albuminous, have been mistaken for cases of acute Bright's disease. Therefore, before I proceed to describe the treatment of Bright's disease, I think it well to point out to you the distinctive features of acute cystitis. Remember that I exclude from our present consideration such obvious and common cases as cystitis from stone, stricture, retention of urine, and gonorrhœa; and I refer to cystitis not excited by any obvious mechanical cause. In a large proportion of cases—in six out of twelve of which I have notes—the disease directly followed, and was probably caused, by a chill. In four cases, dyspepsia with rheumatic or gouty symptoms had preceded the cystitis. In one, the disease came on after feasting with excess of wine. In one case—that of a physician—the symptoms commenced within a few hours after he had been impressed by a peculiar odour from the throat of a boy whom he was attending with a low form of scarlet fever. It is probable that in all these patients the immediate cause of the cystitis was some irritating material in the urine, as acute inflammation of the urethra is excited by the contact of the gonorrhœal poison. In the cases last mentioned, some poisonous product may have entered the circulation, and passed out through the kidneys without exciting disease in them, but setting up inflammation in the bladder. It is difficult to explain or to understand how it happens that exposure to cold should in one person excite acute cystitis, and in another acute desquamative nephritis. Of the twelve cases of acute cystitis which I have noted and tabulated, eight were males, and four females. The ages ranged from seventeen in a female to sixty-nine in a male. In

some cases there has been more or less of vesical irritation for a few days before the acute attack, but in most instances the onset has been sudden and severe. The chief symptoms are frequent micturition, with more or less of uneasiness or pain in the region of the bladder. The calls to pass urine may occur every half-hour, or even oftener; and micturition is usually attended with an increase of pain and a sense of scalding in the neck of the bladder. The vesical irritation is increased by exercise, by exposure to cold, and by alcoholic liquors. The urine quickly becomes turbid with puriform mucus, and often blood-tinged. In one case there was a puriform discharge from the urethra as well as from the bladder, though the disease was certainly not the result of gonorrhœa. Usually the urine has an acid reaction; but, if there be much admixture of blood, the acidity is lessened by the alkali of the blood. It contains an abundance of albumen, partly derived from the puriform secretion, partly from the blood. On a microscopic examination, pus-cells and blood corpuscles are seen in abundance, but no tube-casts. Although the local symptoms are distressing, there is little or no constitutional disturbance. The nights are disturbed by frequent calls to micturate, and the broken rest is attended with a sense of fatigue and nervous exhaustion; but there is little fever, and no vomiting. If, within a few days from the onset of the symptoms, the patient be subjected to appropriate treatment, the disease usually subsides as rapidly as it came on. If, on the other hand, the symptoms be negligently or erroneously treated, the disease may become chronic, and cause prolonged and severe suffering. The urine becomes alkaline, ammoniacal, and fœtid; there are perpetual pain and annoyance; and ultimately the disease may extend backwards through the ureters to the kidneys, and so set up a fatal pyelo-nephritis.

Diagnosis.—The distinction between acute cystitis and acute Bright's disease is sufficiently obvious if you bear in mind that the local symptoms are all referable to the bladder, while dropsy, vomiting, and other renal symptoms, are absent. The urine is usually secreted in normal quantity, and of normal specific gravity. It is albuminous only in direct proportion to the amount of blood and pus which it contains, and the most careful microscopic examination discovers no tube-casts.

Treatment of Acute Cystitis.—In the treatment of this form of cystitis, you have to bear in mind that the inflammation of the lining membrane of the bladder is kept up and increased by contact of the acid and irritating urine. The main object of treatment is to diminish as much as possible the irritating qualities of the urine; with this object in view, the patient must be

confined to bed or to a sofa in a warm room, and be kept on liquid food without stimulants. If there be no special reason in the peculiarity of the patient's stomach to forbid it, milk, cold or tepid, may serve as meat and drink—as a rule, milk is easily digested, and, at the same time, it acts as a diluent. If milk disagree, soup, beef-tea, mutton or chicken-broth may be given, with the addition of some farinaceous pudding, and any simple diluent drink. Pure cold water is as efficacious, though not so pleasant, as aërated or Seltzer water. In addition, the urine is to be kept neutralised by citrate of potash, given every six hours. A warm hip-bath should be given night and morning; a dose of morphia at bedtime, to allay irritation and procure sleep; and an occasional seidlitz powder as a laxative if necessary. Under this plan of treatment, the acute symptoms usually subside with great rapidity; the pain and irritation pass away, and the urine regains its normal characters. After a few days, the citrate of potash may be discontinued, and the tincture of perchloride of iron given in twenty-minim doses two or three times a day after food. So long as any mucus appears in the urine, even though the local uneasiness may have passed away, the patient should be kept under observation and treatment. If a chronic catarrhal condition of bladder remain after the acute symptoms have subsided, copaiba balsam often effects a rapid and complete cure. One capsule may be given an hour or two after food three times a day; and, if the stomach will bear it, the dose may be increased until six and even nine capsules are taken in three doses in the twenty-four hours. In one case which had been of a year's duration, the urine being turbid with blood and pus, and smaller doses of copaiba having failed to cure, at the suggestion of Sir William Fergusson, who saw the patient with me, the dose was increased to three capsules three times a day, and the result was a complete and permanent cure within six weeks; the urine at the end of the treatment being of natural sherry colour, transparent, and without a trace of mucus. Quite recently I have seen a young lady in whom acute cystitis from cold had left a vesical catarrh after a period of six weeks. I prescribed one capsule three times a day, and, in less than a week, the urine was entirely free from mucus, and the cure was complete. I have seen equally good results in other cases. It is probable that the remedy, being excreted by the kidneys, has a local curative action on the mucous membrane of the bladder. The copaiba sometimes brings out a transient erythematous rash.—*British Medical Journal, May* 31, 1873, *p.* 605.

41.—ON THE PATHOLOGY, DIAGNOSIS, AND TREATMENT OF BRIGHT'S DISEASE.

By Dr. GEORGE JOHNSON, F.R.S., Physician to King's College Hospital.

[This paper treats more especially on the treatment of acute and chronic Bright's disease. The origin of the disease appears to be that the kidney becomes damaged by the discharge of its duty, possibly owing to having to excrete abnormal products.]

It follows that a leading principle of treatment is to lessen as much as possible the excretory work of the kidney by instructing the patient to avoid the exciting causes of his malady, by a carefully regulated diet, and by such remedial agencies as experience has proved to be beneficial.

In all cases of *acute Bright's disease*, whatever may have been the exciting cause, rest in bed and in a room of moderate uniform temperature, well ventilated, but without chilling drafts of cold air, is an essential part of the treatment. In a large proportion of cases, rest in bed, with a scanty diet and a liberal use of diluent drinks, will suffice for the cure. A convincing proof and illustration of the effect of exercise, food, and cold, upon the amount of albumen in the urine, is afforded by the fact that, in most cases of albuminuria, the urine passed after rest in bed and before breakfast contains much less albumen than that which is secreted after exercise in the open air and after an ordinary meal.

The diet may consist of milk alone, if milk do not disagree, as it does with some patients. Milk is especially suitable for children; and it serves both for meat and drink, so that no other food or liquid need be taken. It may be taken cold or tepid, from half a pint to a pint at a time. An adult will take sometimes as much as a gallon in the twenty-four hours. Children will take less, in proportion to their ages. If the cream disagree, causing heartburn, diarrhœa, headache, or other symptoms of dyspepsia, the milk may be given skimmed. One reason, amongst others, for giving the milk as a rule unskimmed—that is, with the cream—is, that constipation, which is one of the most troublesome results of an exclusively milk diet, is to some extent obviated by the cream in the unskimmed milk. The advantage of milk as a main article of diet is that, as a rule, it is easy of digestion; and that, taken freely, it supplies a abundance of liquid, which, by its diluent action, has a diuretic influence, and so favours the removal of the dropsy. There are some patients with whom, unfortunately, milk in any form even in small quantities, so decidedly disagrees, that we have to find a substitute in beef-tea, chicken, veal, or mutton-broth, with an egg or two, and some farina-

ceous addition, such as barley-water, arrowroot, rice, or sago, or a small quantity of bread. Under this regimen, adopted and rigidly carried out at the very commencement of acute Bright's disease, the urine soon becomes copious, while the albumen diminishes and gradually disappears, and the dropsy quickly passes away.

In my second lecture, I gave you the physiological explanation of the copious flow of urine which usually occurs during convalescence from acute Bright's disease, and especially when there has been a copious dropsical effusion. This abundant flow of urine usually occurs without aid from diuretic drugs, or, indeed, from drugs of any kind. Stimulating diuretics, such as squills, cantharides, or turpentine, would be injurious by increasing congestion of the kidney. The best diuretics in cases of acute Bright's disease are those means which tend to lessen congestion of the kidney; such as dry cupping or hot poultices or fomentations over the loins, warm baths, and a free use of diluent drinks, one of the pleasantest and most efficacious being the "imperial drink," made with cream of tartar and lemon.

Warm baths are particularly useful in the early stage of an acute attack, and more especially when exposure to cold has been the exciting cause of the renal disease. A warm water bath, at a temperature of 98 or 100 deg., may be given every night during the first few days of an acute attack; or a hot-air lamp-bath; or, what I believe in most cases to be still more efficacious, a wet sheet and blanket bath. A sheet is wrung out of warm water; and the patient, either naked or covered only by his shirt, is enveloped in the wet sheet up to the neck. Then three or four dry blankets are closely folded over the wet sheet. He may remain thus packed from two to four or six hours, or even longer. Recently, a boy in the hospital with acute renal disease and almost complete suppression of urine, consequent on scarlet fever, was kept packed incessantly for four days without serious discomfort, and with great relief from very distressing and alarming symptoms. When he left the hospital, all traces of his malady had disappeared. If the packing be long continued, the sheet has to be re-wetted as soon as it becomes dry. The evaporation and consequent drying of the sheet will be slow in proportion to the closeness of the blanket packing. If the outer blanket be covered by a mackintosh cloth, the sheet remains wet for a much longer time than when no waterproof covering is used; but patients often complain of a feeling of oppression when surrounded by the impervious mackintosh. The advantage of the blanket-bath over a warm water or a hot air bath, is, that it requires no special apparatus, that the diaphoretic action may be more

prolonged, and that in most cases it is more agreeable to the patient. The hot air bath not unfrequently causes an unpleasant throbbing in the head, or a feeling of exhaustion and even faintness. When the wet pack is removed, the patient should be quickly rubbed dry, and enveloped in dry blankets.

The diaphoretic action of any form of warm bath is assisted by copious libations of simple diluent drinks, and it may also be aided by the internal administration of the solution of acetate of ammonia. It has been objected on theoretical grounds, that to promote perspiration in these cases is injurious by diverting to the skin the water which is required to wash out the uriniferous tubes. To meet this objection, you have only to bear in mind that the dropsical patient is oppressed by an excess of water, which has been poured into the areolar tissue and the serous cavities, in consequence of defective urinary secretion resulting from the inflammatory engorgement and obstruction of the kidneys. If, therefore, by the relaxing effect of external warmth, you divert a large amount of blood to the surface, you thereby lessen the congestion of the kidney, increase the freedom of the renal circulation, and so favour the occurrence of that copious secretion of urine which is one of the surest signs of satisfactory progress, and by which the uriniferous tubes may be effectually flushed and cleansed. Whatever fluid is lost by perspiration, may be quickly restored by the liberal use of diluent drinks, which again assist the secretory activity of both the skin and the kidneys. In fact, one of the main objects of treatment is to increase the freedom of the circulation, more especially through the kidneys; and thus to get rid of the excess of stagnant water which has accumulated in consequence of defective action of the skin and kidneys.

Purgatives may be usefully combined with other means for lessening dropsical effusion. In ordinary cases of acute Bright's disease, I do not, as a rule, advise the frequent employment of drastic purgatives. I reserve this method of treatment for cases in which there is an excessive and increasing dropsical effusion which does not yield to other means of cure, but more especially for cases in which cerebral symptoms, the result of uræmia, are either present or apparently impending. Dr. Abercrombie and others, who wrote on brain-disease before Dr. Bright's discovery had led on to our present knowledge of uræmic nervous symptoms, published cases of cerebral disease which they took to be of an inflammatory or an apoplectic character, in which the most strikingly beneficial results were obtained by free purging. Dr. Abercrombie, in discussing the treatment of inflammatory affections of the brain, states that, according to his own experience, "more recoveries from head-affections of the most alarming aspect take place under the use

of very strong purgatives, than under any other mode of treatment." Our more recent experience is entirely confirmatory of Dr. Abercrombie's statement; but our improved pathology enables us to add a very important qualification—namely, that most of the cases in which formidable cerebral symptoms have been removed by the action of strong purgatives, have been neither inflammatory nor apoplectic in their nature, but cases in which brain-symptoms have resulted from blood-poisoning; and in the majority of instances the poison has been uræmic. Amongst the cases recorded by Dr. Abercrombie as examples of inflammation of the brain successfully treated, there are two in which the brain-symptoms were associated with anasarca, which had followed an attack of scarlet fever. These were unquestionably cases of acute renal disease, with cerebral symptoms of uræmic origin. I offer for your practical guidance this rule of treatment: when such symptoms as headache, delirium, convulsions, or coma, are the result of uræmia, give purgatives freely; and, if the renal disease be acute, and therefore probably curable, your treatment will often be completely successful. On the other hand, when you have reason to believe that the like brain-symptoms are consequent on cerebral hemorrhage, or embolism, or thrombosis, be very cautious in the use of purgatives, which may greatly increase the patient's distress and exhaustion, while they can do little to improve his condition. In inflammatory affections of the brain and its membranes, purgatives are often useful, but less frequently and strikingly so than when cerebral symptoms are the result of uræmia. As to the form of purgative in uræmic cases, croton oil is the most convenient when there is coma and consequent difficulty in swallowing a more bulky dose. When there is no such difficulty, two pills, composed of three grains of calomel with seven grains of compound colocynth pill, may be followed in four hours by an ounce of the compound senna mixture; or the following powder, which I think an improvement on the compound jalap powder, may be given: ℞. Scammoniæ resinæ gr. v to viii; potassæ tartratis acidæ ℈i; pulveris zingiberis gr. iii. Misce. The dose to be repeated once or oftener, according to circumstances.

When, in a case of acute Bright's disease, the renal congestion is excessive, as shown by the scanty secretion of highly albuminous urine, with vomiting, headache, and other threatening nervous symptoms, local bleeding by leeches or cupping on the loins is often extremely useful, and is quickly followed by an increased secretion of urine. If, by the abstraction of a few ounces of blood from the loins, we relieve the renal congestion, we thereby lessen the destruction of blood-constituents which results from contamination of the blood by urinary excreta.

Moderate and timely local bleeding, therefore, tends to economise blood, and to prevent its waste.

The more I have seen of the disease, the more hopeful I have become as to the ultimate result, when the history and the symptoms, and, above all, the chemical and microscopical characters of the urine, do not indicate extensive and irremediable degeneration of the kidney. In all the cases of recovery from long continued albuminuria, the preparations of iron have entered largely into the medicinal treatment of the disease, and have apparently contributed much to the favourable result. There are two preparations which I have found especially useful; these are the tincture of the perchloride and the syrup of the phosphate—the former in doses of from fifteen minims to half a drachm, and the latter in drachm doses twice or thrice daily. The preparations of iron are best taken soon after food. I have frequently combined with each dose of the perchloride of iron ten grains of hydrochlorate of ammonia; and I believe that this ammonio-chloride of iron is an useful combination.—*British Medical Journal, June* 21, 1873, *p.* 696.

SURGERY.

FRACTURES, DISLOCATIONS, AMPUTATIONS, & DISEASES OF BONES, JOINTS, ETC.

42.—ON NERVOUS MIMICRY (NEUROMIMESIS) OF DISEASES OF JOINTS.

By Sir JAMES PAGET, Bart., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

[Sir James Paget has been giving us some most interesting lectures on the nervous mimicry of organic diseases, showing that often the pains in the nerves of a part will give rise to many of the symptoms of real disease when in fact there is very little the matter with the case. In this lecture he gives us some capital hints on joints.]

Among all the joints, the hip and the knee, which are the most frequent seats of real disease, are equally so of the mimicry: a fact not easy to account for. It may be due to mental association, perhaps unconsciously; or to a mingled inheritance—for instance, to an inheritance of nervous constitution and of relative weakness in the joint or joints most weak in progenitors. After the hip and knee follow, in order of frequency, the tarsal joints and carpal, or the elbow and shoulder; but, in these, mimicries are too rare for counting.

Another fact, adding to the difficulty of diagnosis, is that the most frequent exciting causes are the same for the real and for the imitated affections of the joints. Injury, or some great fatigue, is commonly referred to as the source of the mischief. The injury may seem inadequate for such trouble as followed; but you cannot rely on this. Many of the worst instances of scrofulous arthritis follow injuries that seemed very trivial. It may suggest a suspicion of neuromimesis if pain set in with full severity directly after an injury that was not severe; but the suspicion must be lightly held. One of the most acute inflammations of the hip-joint I have seen set in severely, with an almost agonising pain, directly after a wrench of the joint in quickly turning round.

Thus, then, you can get little help for diagnosis from either the seat or the apparent cause of the malady; they are for the

most part the same for the real and for the mimic affection. Your reliance must be on the examination of all the features of each case, and on a right estimate of the weight to be attached to each. Let me then take in turn each of the signs of inflammatory affections of joints, and see how far they may be imitated, whether separately or together. For although it may be said generally that an inflammation of a joint should be marked by many signs, and that in a well-marked case you may study them all, yet in practice you cannot treat lightly any case which has even one clear sign of diseased joint; for this may be only the first sign, which others will follow, or the last, which has survived the rest; or it may be one which is so exaggerated as to conceal the others. What is wanted in every case of suspected disease is, that you should be able to say positively yes or no, and this you cannot do without knowing the weight in evidence of each usual sign.

First, as to pain. Alone, it is not to be at all relied on for a sign of inflammation of a joint, especially if it be severe. If a patient be ready to scream when the accused joint is touched, and yet the joint is not over warm and the patient not feverish, you may be nearly sure of neuromimesis; and more nearly still if the pain be rather in the parts outside or about the joint than in the joint itself, so that a light touch is said to hurt as much as a hard one, or a pinching of the skin as much as pressure on the joint itself. You must not even rely on what are supposed to be characteristic pains, such as those felt at the knee for disease of the hip, or about the middle of the arm for disease of the shoulder, or even those grinding and burning pains at night which some regard as characteristic of ulceration of cartilages. All these may be mimicked.

I had a boy, aged about 15, in the hospital, who had these night pains in a most marked degree in one knee, and the joint was a little swollen; and he, being tuberculous, was wasting, hectic, and very ill. I did not doubt that he had destructive articular disease, and the use of the actual cautery cured his night-pain; another reason, some would have said, for being sure that his articular cartilages were ulcerating. Soon after this he had similar pains in or near the tibio-fibular joint, and these also were cured with the cautery. But he went on and died with pulmonary tuberculosis, and I examined his knee-joint, and found it almost healthy. A very small piece of one edge of the cartilage on the femur appeared eroded, but it was a trivial change, and quite inadequate to account for the severe pain of which the lad had long complained.

The case had been one of nerve-pain at the joint, and the imitation of organic disease had been made closer by the signs of the coincident disease of the lungs. But for this, it might

have illustrated what you may take as a general rule, that acute pain in a joint, if it has existed for even a few days without either local or general increase of temperature, is not a sign of acute inflammation of the joint. It may be rheumatic disease or rheumatic gout, or some slight inflammation after injury, in a neuralgic person, but it is not acute inflammation.

There is more difficulty in judging of the meaning of pain in a joint when it is not severe, but dull, aching, "wearing," as patients describe it. You must be cautious in these cases. Pain alone is not enough to prove organic disease; yet the lower degrees of pain seldom exist constantly and long without some organic mischief. I have indeed known several such cases, especially of pain at the hip, the conclusion of which made me believe that the pain was only nervous, for the patients got at last suddenly or very quickly well, without stiffness or other apparent change at the joint, or with a shifting of pain to some other part. But in all such cases you must be cautious and watch for other signs of disease adding themselves to the pain, such as local heat, swelling at the joint, wasting of parts about it, and others that are least dependent on the sensitive nervous system.

The pain I have been speaking of is that which may be felt in the joint even while it is at rest. Different from this is the pain which prevents the free movement of the joint. It is observed alike in the real and the mimic disease; and you may often judge the pain to be mimicry by its inconsistent severity. If with scarcely any other sign of disease a joint will not permit the slightest movement because of pain, you may greatly suspect the reality of disease; but be very careful not to overlook the signs of increased pain on movement in cases of slight real disease, especially in the diseases of the hip in children. You may often find this the only sign making it clear that a child has real hip-disease. A suspected joint may allow free and smooth and painless movement till, for instance, in extreme abduction of the thighs one adductor becomes much tenser than the other, or in extension the loin is quickly raised, or by some other movement, it becomes evident that the joint will not allow extreme movement without pain, although within all but its extreme range its movements may be free and painless.

In these, among other cases, chloroform or ether may help in diagnosis. In the real disease, as in the mimic, while the patient is utterly insensible, the joint may be moved as widely as in health, unless indeed there be such changes in its structure as might alone have proved its disease; but commonly you will observe that, in the real disease, the muscles become alert, and restrain the movement of the joint before the patient has

regained consciousness; while in the mimic disease there is no restraint till consciousness is completely regained. The test is a delicate one, but I think I may be sure that it is a true one, and fit to be relied on, whenever the chief sign of disease of a joint is a restraint of movement on account of pain and the guarding action of muscles.

Closely allied with this pain on movement of an inflamed joint is its stiffness, with contraction or other set posture, dependent on muscular action; for this posture, whether it be due to choice or to reflex movement, is the posture of greatest ease, or of safest guard against weight or shock or other causes of pain. The absence, therefore, of the fixed, or nearly fixed, posture usually observed in a diseased joint, may always suggest the suspicion of mimicry. It would be rather strange to see a hip or a knee extended after many weeks of such pain as would be felt in an acute arthritis, unless, indeed, they were rheumatic or gouty, with exaggerated pain, or had been carefully maintained in good position. It would require the presence of many other signs of real disease to counterbalance the absence of this sign; for diseased joints, left to themselves, will be habitually, or always, in the position easiest to the patient.

But the reverse of this is not true. Very commonly a joint mimicking disease assumes the posture of disease—assumes and maintains it stiffly in even an extreme degree. This may be seen even when there is no objective pain in the joint; but much more, when the joint is a little really painful, as after a blow or strain, the nervous condition of a patient may either make this pain so intense as to demand the position of greatest ease, or may bring about this position for the relief of even a little pain. Especially the posture of hip disease is apt to be imitated by the drawing up of one side of the pelvis and rotating it, so that the limb looks shortened.

The conjunction of pain and stiffness in a joint always looks like real disease; but you may generally detect the mimicry by observing that, while these things would indicate disease of much severity, everything else is as if there were no disease, or at most only a very mild arthritis. The inconsistency of the several parts of the case exposes its true nature. Lately I had to see a young lady, with a reported healthy nervous system, whom I found lying in bed with extreme contraction of the thigh towards the pelvis, pain at the hip and knee, increased pain on any touching near the hip-joint, and especially great pain and tenderness at and about the tuberosity of the ischium. She could not bear the least attempt to straighten the joint, and the contraction was said to be persistent during sleep. She had had vomiting, hated food, and looked ill and distressed;

and all these troubles were ascribed to slight injury or over-fatigue a few days previously. The case looked very like real disease of the hip; but if it had been so at all it must have been a very acute disease, swift and severe, such as should have had attendant fever. Yet the pulse and temperature were natural, and there was certainly nothing in the case which might not be explained by mere nerve-disturbance. And the event proved that there was no organic disease, for after a few days' rest, with careful food, and some wine, and mental quietude, the pain ceased, and then very slowly the limb resumed its natural power and posture, and the young lady married.

Let me here tell you of two or three notable groups of cases of pain and stiffness of joints without real disease. None are more frequent than those of boys from ten to fifteen, who complain of pain and declare themselves unable to walk after injuries of the ankle or knee, or who disuse their arms for pain after injuries of the elbow or other part. They describe the pain as horrible, and hold their limbs in some unnatural position, and limp and cry when you try to make them walk; yet you can find nothing wrong in the shape or size, or temperature of the joint, or in the general health. It is very hard to say whether they are shamming or neuralgic; but the utter inconsistency of their cases proves that they are the one or the other, and you must make them use their limbs. They will often do it better if you have first moved the joint severely for them, and "loosened" them.

Something like these, yet different in having little or no pain, are the children who mimic a disease of a joint through fear. After a hurt, the immense care bestowed on a joint, and the earnest injunctions not to move it, seem to impress some children's minds so deeply that, long after all is well, they hold up their joints scrupulously and timidly, and dare not try to use them. You may amuse yourselves with the astonishment with which both child and parents find, on a positive assurance, that walking, or any other use of the lame limb, is quite easy and painless.

Another group allied with these is, that in which you find young people with joints contracted by involuntary and quiet muscular power after injury. The joints are painless, unless when great force is used at them; and you can easily feel that their stiffness is not through inflammation or adhesion, but muscular resistance like that which sometimes produces wry-neck directly or very soon after a blow. You feel a kind of elastic

recoil at efforts to move them, as if the effort was resisted by a tough elastic substance. Ether or chloroform decides at once the diagnosis. As soon as the patient is insensible, the joint becomes moveable as widely and as smoothly as an uninjured healthy one, and only slowly, if ever, stiffens again.

Just like these are the ordinary cases of painless stiff joints in hysterical girls,—the muscles hold them fixed, and that is all; the joints are healthy to the touch and the sight, and even to the patients are painless unless violently used.

In all these cases the ground of diagnosis is essentially the same. You have one or two or three signs of the disease of a joint present in an extremely marked degree, or at least well marked; a pain or a stiffness, or both, and limping or other consequent disability, such as would exist in a joint severely or long diseased. But with a joint really thus diseased there should be—unless it had some chronic, rheumatic, or gouty trouble—swelling, or heat, or spoiled shape, or general wasting of the limb, or all these together; and with them usually some disturbance of the general health. The absence of these is weightier evidence than the presence of the other symptoms.

All that I have just been saying relates to the distortion of joints produced by muscular action, not to deformity due to displacement of one or more of the bones forming the joint, such deformity as you see, for instance, in the knee when the tibia falls backwards and outwards from the femur, or is rotated by the weight of the foot, on which also the bedclothes may have been allowed to rest. When this, or any similar deformity exists, it is a nearly sure sign of real disease, past or present, for it can scarcely happen except through textural changes, through softening of the ligaments and other textures about the joints permitting one bone to move away from the other. Now, such softening as this can hardly take place except in inflammation. I will not say that it is impossible, but I know that it is very rare. Posture alone, though very long continued, will not produce deformity of joints with displacement of bones. I saw a gentleman who, in a half-lunatic condition, sat for five years in the same posture, and was credibly said to have never moved. At the end of this time his knee-joints were contracted to a right angle, and felt as if absolutely fixed; yet they were not deformed. Their bones had their right relations; and, after some weeks of extension with instruments, the knees were straightened, and power over them was completely gained.—*Lancet, Nov.* 1, 1873, *p.* 619.

3.—ON THE TREATMENT OF FRACTURES OF THE FEMUR BY A NEW METHOD AND APPARATUS.

By Dr. ZACH. JOHNSON, A.M., Surgeon to the Kilkenny County Infirmary.

The management of a fractured thigh is beset with difficulties known to every experienced and practical surgeon, and it cannot be said that any of the apparatus heretofore devised completely or satisfactorily fulfil the intentions of the inventors or the requirements of the practitioner. It will not, then, be considered presumptuous to bring under notice, in addition to the many in use, one which has been subjected to the test of hospital experience for many years, and has been found to possess advantages over its rivals, which the following description of its principle and construction will explain.

It is not proposed to enter here into a complete dissertation on the treatment of fractured thigh, and the various plans from time to time proposed and recommended by eminent surgical authorities, nor to examine into the respective merits of the inclined and extended positions. With the received opinions on these points it is presumed the readers of this article are already acquainted. The arguments of the late lamented Professor Houston, so lucidly propounded in his admirable little *brochure* on the advantages of the extended position in treatment of fractures of the lower extremity, leave nothing to be said on this point. As a rule, then, the extended position is that now accepted as the best, when practicable. The cases are unusual which do not admit of it, though it may here be incidentally observed that there are two in which it is not available, and though these are rare, as they occasionally present themselves in actual practice, they should not be overlooked. The one is where the fracture occurs in the upper third of the femur, in which case the upper fragment may be tilted up, almost at a right angle, by the spasmodic action of the psoas and iliacus muscles. Here there is no resource but the inclined plane for some days, until the irritability of the muscles has been overcome, when the limb may be gradually brought down to the horizontal position, and the extended plan of treatment ultimately put in practice. The other where, with a knee anchylosed at an angle, the femur is broken; and here, obviously, the inclined plane alone is available throughout.

It is, however, with the extended position we have more immediately to do. It is for the more effective management of the limb in that position the apparatus about to be described was specially devised and subsequently tested, although it will be seen by-and-by, it is capable, on an emergency, of being adapted for use in the inclined position also.

The objections to Desault's long splint, with its oblique inguinal and ankle straps, its defective extensile powers, and tendency to excoriate the perineum and groin, are too well known to require further comment. The paramount objection, obviously, is the loss of power arising from the obliquity of the extending and counter-extending forces. The minor objections are several, such as the danger of excoriation of the groin and perineum above, and the ankle and instep below, interference with the use of the bed-pan, &c. &c. These objections all hold good, more or less, in the case of Liston's and Bulley's splints, which, although they present some advantages, retain many of the most serious defects of Desault's apparatus.

Our primary object in the management of a fractured thigh is, that the extending and counter-extending forces should antagonize each other in a *direct* line, which line should be parallel to the long axis of the limb. On this principle depends the efficiency of all our expedients to counteract the inevitable tendency to shortening, caused by the contractions of the powerful femoral muscles. Here it is that Desault's plan fails, the obliquity of the antagonizing forces necessarily entailing great loss of power. Next in importance is the condition that the power should be constant and persistent, such as to weary and overcome the rebellious muscles, while our aim should be to accomplish this in such a way as shall be thoroughly efficient and at the same time in the least possible degree distressing or inconvenient to the patient. With a view to devise means by which to keep up a direct extending and counter-extending power, various expedients have been proposed. For this purpose the weight and pulley was long since adopted and has had very gratifying success in the hands of many practical surgeons. It has been frequently used with satisfaction in the Kilkenny County Infirmary by my colleague and myself. About the year 1852, I published, in the Dublin Medical Press, the details of a most remarkable case where a femur, shortened after a first fracture, was brought to its normal length by this treatment, when accidentally broken a second time, about a year or more afterwards; and Dr. Smith, of Donoughmore, has drawn attention to the fact that the weight and pulley was used to effect extension of the fractured thigh, so far back as 1829, by Dr. Daniell, of Georgia, in America, and by Sir P. Crampton, in the Meath Hospital (Medical Press and Circular, 23rd June, 1869).

But the weight and pulley, however effective and admirable in its action, has its objections, as every practical surgeon must have found. The difficulty of effecting counter-extension, the tendency to drag the patient to the foot of the bed, intolerance on the part of the patient, generally speaking (if the weight be

sufficient to overcome the contractile power of the muscles), all present difficulties which stand much in the way of its universal or even general adoption. Besides it is awkward, unseemly, and inconvenient.

It will be admitted that any apparatus which could embody all the advantages of the several systems now in use, at the same time that it got rid of their several disadvantages, would be a great boon to the practical surgeon, as well as to the sufferer. With such intentions the apparatus about to be described has been constructed. It has been tested amply in actual practice, and the result has been to induce me to submit it to the experience and judgment of the profession.

Among the advantages claimed for the apparatus now proposed, is its simplicity; although a modification of Desault's, Liston's, and Bulley's splints, combining the good points of each, with the addition of several innovations unknown to them, it is simple, easy of application, and, best of all, thoroughly effective. It does not excoriate, it can be so used as not even to incommode the patient, while it confers on the surgeon power, which no apparatus hitherto in use has realized.

The apparatus consists of a single long splint and foot-piece, each of which requires separate description.

The long splint resembles Desault's inside splint, but differs from it in some very essential points. The most important of these is the method of effecting the counter-extension. Instead of the perineal strap, which is open to so many objections, the upper or perineal extremity of the splint, which is slightly concave, is applied directly to the perineum. It is softly padded on the extremity with caoutchouc, which again may be covered with chamois, silk, lint, or any other movable and non-irritating material. The ramus of the pubis is here made the

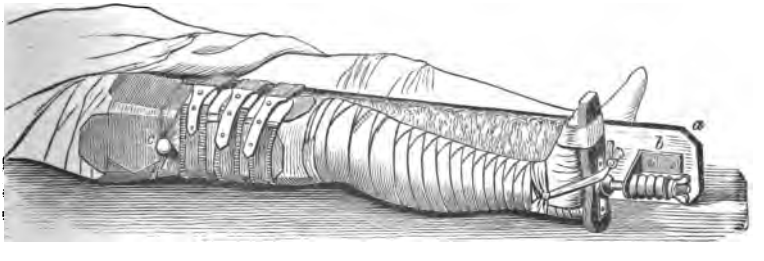


Fig. 1.—Splint applied on inner side of limb. Ramus of pubis the fulcrum of counter-extension. Foot-piece parallel to splint. *a*. Splint. *b*. The standard or fulcrum extension. *c*. The button by which counter-extension is effected, when splint is applied on outer side of limb.

fulcrum, and it will be seen that, with the foot-piece used, the extending and counter-extending powers act "in directum." The pressure of the splint against the perineum is borne, so far as past experience has gone, without complaint, and has great and obvious advantages over the perplexing and ineffective oblique inguinal strap of Desault's splint. (See Fig. 1.)

But, should any objection arise to using this fulcrum, a second one has been provided, the idea of which has been borrowed from Dr. Sayer's ingenious method of treating affections of the hip-joint and knee.

A short splint of strong sole leather or flexible material, about some twelve or fourteen inches long, is provided with a stud or gig-apron button, which is firmly rivetted; this splint is to be attached to the outer side of the thigh and pelvis. This can be effectively done by means of oblique adhesive straps, of sufficient length and breadth, laid on in the manner proposed by Dr. Sayer in attaching his apparatus for extension of the hip and knee. The surface of the splint itself may be made adherent by a coating of starch, or soap plaister, and by these means a degree of fixity can be obtained more than sufficient for any practical purpose. (See Fig. 1 *c*.)

In the upper end of the long splint may be seen a series of button-holes, capable of fitting on the stud in the short splint. By transferring the long splint to the outer side of the thigh, the surgeon can now, if he wishes, make this stud the point of counter-extension, an arrangement which has been found perfectly effective, and which contributes immensely to the comfort of the patient. (See Fig. 2.)

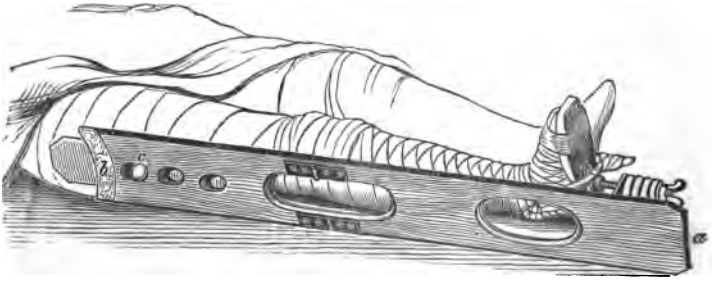


Fig. 2.—Splint transferred to outer side of limb. Fulcrum of counter-extension now the button *c*, acting through whichever button-hole best suits the length of the limb. *b*. The padded end, which rests against ramus of pubis, when splint is applied on inner side. In this diagram the bed has been tilted up, so as to bring more into view.

The fulcrum of counter-extension having been decided on, extension is to be effected by means of the foot-piece attached to the other end of the long splint, to the peculiar construction and advantages of which attention is now directed.

By reference to Fig. 3, it may be seen that, by means of a ball and socket joint (*b*) at the back of the foot-piece, it is capable of an infinite variety of adaptations, while, by the use of a pinching screw (*c*) provided for the purpose, it can be firmly and immovably fixed in any required position. The ball is rivetted on the end of a bar (*d*), which moves horizontally, backwards or forwards, through a mortice hole in the standard, or support (*a*), which is affixed to the long splint. On the other extremity of the horizontal bar a winged nut (*e*) plays, drawing the foot-piece towards the standard, or support (*a*), with great power when required. Between the winged nut and the standard may be observed a series of caoutchouc rings or flanges (*f*), the use of which will be explained presently. (See Fig. 3).

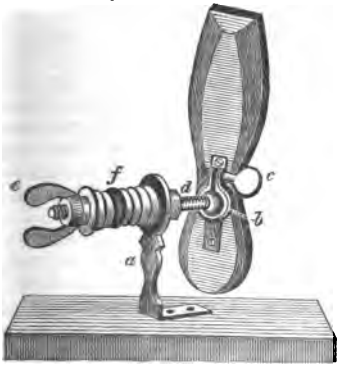


Fig. 3 —Separate view of foot-piece:—*a*. The standard or fulcrum of extension, which admits of being fixed either perpendicular or parallel to the bed. *b*. The ball and socket joint, by means of which the foot-piece can observe any position with reference to the standard *a*. *c*. The pinching screw of the ball and socket joint, which fixes the foot-piece immediately in any required position. *d*. The traversing bar, which runs backwards and forwards through the mortice hole in *a*. *e*. The winged nut, which plays on the thread cut on the corners of the square bar *d*. *f*. The caoutchouc flanges, which, when compressed, draw the foot-piece towards the fulcrum *a*.

The advantages of this foot-piece are numerous and obvious. By attaching the foot to it any requisite position may be obtained, and then maintained by a turn of its pinching screw. This is of immense value in injuries of the leg and ankle. The motions of the ball and socket joint allow of any adaptation, and the pinching screw secures it. In fracture of the thigh it obviates eversion of the foot. In fracture of the fibula it can evert the lower fragment.

If we suppose the extending lacs to be attached to the limb, in whatever manner the surgeon may prefer, and the foot-piece brought into apposition with the sole of the foot, by sliding the horizontal bar through its standard (fig. 3, *a*), the lacs are to be tied firmly at the back of the foot-piece, and

now extension will be effected by screwing up the winged nut, which draws the foot-piece towards itself with irresistible force.

Thus two important objects are obtained—a steady, gradual force of great power, and one acting in a direct line with the axis of the limb.

It will be observed, that by a simple revolution of the foot-piece on the ball and socket joint the long splint may be applied indifferently to either the outer or inner side of the leg.

Having obtained a powerful extending force, acting "in directum," it remained to render this force persistent and self-adapting, so as to combine, if possible, the action of the weight and pulley with the advantages already gained.

This has been effected in the following manner:—

When tension is applied by so screwing the winged nut so as to draw downwards the foot-piece, the caoutchouc flanges, which lie between it and the standard (and to which attention has been already drawn), become compressed, and by their elasticity exercise a constant strain on the extending lacs, which can be modified by increasing or relaxing the pressure. Thus the antagonizing action of the muscles may be wearied out. The more they contract the greater the strain on the lacs, while the tension continues, in modified degree, in proportion as they yield and relax, and thus the action of the weight and pulley is emulated; and if the attachments of either extending or counter-extending agents should yield or slip, the resiliency of the caoutchouc will compensate for the derangement, and still keep up an equable tension.

Such are the paramount and essential features of the apparatus. Some points of detail remain to be noticed and explained.

The reason that the horizontal bar is square, is to ensure steadiness when it is desired to fix the foot in any peculiar attitude, by tightening the pinching screw of the ball and socket joint; were it round, the foot-piece could revolve, when it was desirable to have it fixed and steady. The thread of the screw which the winged nut traverses is traced only on its corners, but will be found as unerring and effective as if the circle were complete.

The persistent tension of the weight and pulley might be simulated by elastic lacs, secured to the foot-piece, but with this great disadvantage, that when the lacs yielded the foot would be drawn away from the foot-piece, and would be free to take wrong positions. It is of manifest importance to keep the foot in close apposition with the foot-piece by a slipper, bandage, or adhesive straps, so that when any peculiar position has been attained, deviation from it may be obviated. In order to

ensure this it was necessary so to arrange the elastic force that, while it yielded to any extraordinary tension, the foot should not become in the least separated from the foot-piece, and its requisite attitude should be still perfectly maintained. This has been accomplished by interposing the elastic medium between the extending agent (the winged nut) and the fulcrum against which it works (the standard attached to the long splint). (Fig. 1, *b*.)

But as cases will occur which cannot be treated in the extended position, such as those previously alluded to, arrangements have been made so as to allow of converting this splint, on emergency, into a double-inclined plane.

A large oval fenestrum may be noticed about the middle of the long splint. This allows of the splint being kept in close apposition with the thigh, so as to avoid encroaching on the scrotum or vulva, without pressing against or hurting the knee, opposite to which this vacancy has been purposely constructed. It will be also observed that at the centre of this oval the splint is hinged so as to allow of its being doubled together on the outer side. This not only makes the entire more portable, but allows of its being converted, at any moment, into a double-inclined plane, the inclination of which can be easily regulated by pillows or cushions. The foot-piece now revolves to a suitable position, perpendicular to the plane of the leg-piece, instead of being parallel to it as before, while a second oval has been removed, to secure the heel from any contact or pressure. Thus, in case of emergency, a double-inclined plane can be readily extemporised.

When it is desired to keep the long splint straight, for the purposes of extension, the yielding of the hinge can be effectually prevented by inserting a pad of adequate thickness between the knee and the site of the hinge, which will prevent the bending of the splint, whether it be on the inner or outer side of the thigh.

The revolving foot-piece will be found of great advantage in other cases besides those where an extending power is required. In accidents and injuries of the leg and disease of the ankle, where it may be of importance to secure some special position of the foot, it will be found most useful. Here it is to be kept in apposition with the sole of the foot by means of a second pinching screw, whose office is to prevent the square bar from sliding in the standard, and so to keep the foot-piece permanently in contact with the foot, which now derives support from it. If it be desired to effect any peculiar position of the foot, the ball and socket joint enables the foot-piece to be adjusted at any fraction of an angle, and the foot having been previously secured suitably by adhesive strapping, the entire

may be fixed in the required posture by a turn of the pinching screw (fig. 3, *c*) which controls the ball and socket joint.

This foot-piece is equally useful for support or extension. It is applicable alike to long splint or inclined plane. It was exhibited by me at the Surgical Society some twenty years ago, since which time it has been amply tested in hospital and private practice. It has been found to possess great advantages in a variety of cases. Where some special position of the foot is required it can exercise either extension or support, as may be most desirable. In the case of fractured fibula it can fulfil the purposes of Dupuytren's splint. For the purpose of direct extension, it has proved more effective than any means hitherto proposed.

With respect to the best method of applying extension in the case of fractured thigh, it may not be amiss to say a few words. It can be applied, as is so frequently done, above and close to the ankle, by means of the familiar clovehitch tie. Here it is apt to excoriate and cause pain by its pressure on the malleoli. A great advantage will be gained by attaching the extending agent near the knee; this may be accomplished by broad adhesive straps, applied after the method followed by Dr. Sayer in his treatment of hip disease. By this means double points of attachment may be effected by which a double advantage will accrue. Thus one attachment may be arranged above, the other just below the knee, and by using these alternately the patient obtains great relief. It will be found that the irritability of the femoral muscles will be lessened by applying the extending power high up. When practicable. it is well to attach it above the knee, and relieve the ligaments of the knee from strain, but in any case it can be attached, with advantage, above the calf of the leg.

A decided advantage will also be obtained from the use of short secondary splints, embracing the femur alone. Of these, one in front and one on either side will be sufficient. They may be secured by broad straps and buckles, and their equable pressure tends to soothe the spasmadic irritability of the femoral muscles. Their use was much insisted on by the late Sir Philip Crampton, at the Meath Hospital, and they have been justly recommended by other eminent authorities.

When counter-extension is made against the ramus of the pubes, and the long splint applied to the inner side of the leg, it serves as the inner splint, while the leather splint, with stud affixed, is available as the outer one. The straps and buckles may now embrace all, and by including the long splint will keep it close to the thigh, and prevent any encroachment on the scrotum or vulva.

When counter-extension is effected by the button on the

outer side of the thigh, it will not be so convenient to include the long splint in the straps which confine the short ones. The comfort enjoyed by the patient when this method of counter-extension is used is very great. It can be best appreciated by applying extension on the inner side first, and then changing to the outer side. It is fully as effective as any other method, and presents no interference whatever with the use of the bed-pan; thereby contributing materially to the convenience and comfort of the patient.

It would not be right to conclude these observations without acknowledging my obligations to the ingenuity of Dr. Sayer, of the Belle Vue Hospital, New York, from whose method of affixing his apparatus for hip and knee disease I took the idea of attaching a fulcrum (fig. 1, *c*) on the outer side of the thigh, a measure which conduces vastly to the comfort of the patient. The mode of applying counter-extension by broad adhesive straps, applied above and laterally, to the calf of the leg, and to which the name of "Pancoast's Stirrup" has since been given, was adopted by me previous to 1850, and referred to in my publication "On the Weight and Pulley," about that time.

I now submit this mode of treating disease and injury of the lower extremity with all deference to the judgment of an impartial profession.—*Dublin Journal of Medical Science, Aug.* 1873, *p.* 101.

44.—SUCCESSFUL CASE OF EXCISION OF THE SCAPULA.

By JAMES SPENCE, Esq., F.R.S.E., Surgeon to the Queen in Scotland, Professor of Surgery in the University of Edinburgh.

[The patient, aged sixty-eight, was admitted to the Royal Infirmary on the 25th of December 1871, on account of a tumour in the scapular region. The swelling had existed upwards of twenty-three years, but had given him no pain or inconvenience until about six months before his admission, when it began to increase rapidly in bulk and also became painful.]

On examination I found that whilst the part of the tumour in the infraspinous fossa of the scapula was still pretty movable and not much altered in size or form, there was a very marked enlargement towards the axilla, where it pressed forwards, surrounding the neck of the scapula and the shoulder-joint, and nearly filled the axillary space. At the neck of the scapula the growth seemed fixed. I therefore gave it as my opinion that excision of the scapula would probably be re-

quired. As the patient was desirous that the operation should be performed as soon possible, I proceeded to operate on February 21st.

Operation.—As the tumour had originally been of slow growth, and had remained seemingly movable up to a recent period, I thought it advisable, before proceeding to excise the whole scapula, to make my incisions so as to ascertain whether it was firmly attached to the bone or merely bound down by muscles and fascia. I accordingly began by making an incision from the posterior part of the acromion process obliquely downwards and backwards over the tumour to beyond the inferior angle of the scapula, dividing freely the fascia and fibres of the infraspinatus muscle. This exposed the scapular aspect of the tumour, smooth, defined, and so movable that it seemed as if it would have been easily turned out; but at the neck of the scapula and around the glenoid cavity the growth was evidently from the bone or periosteum, and was soft and cerebriform in substance, so that I proceeded to excise the whole scapula. With this view I entered the point of my bistoury over the clavicle internal to the attachments of the coraco-clavicular ligaments, and carried the knife backwards over the acromion into my first incision, so as to complete a long incision from over the outer part of the clavicle to the inferior angle of the scapula. I next made an incision from the superior angle of the scapula obliquely downwards and forwards over the tumour towards the neck of the scapula. The flaps of skin were then dissected and reflected, and the attachments of the trapezius and deltoid muscles to the spine of the scapula divided. I then cleared the clavicle by separating the attachments of the trapezius, deltoid, and great pectoral muscles, and divided the bone, dissecting through the coraco-clavicular fascia, so as to enable the axillary artery to be completely commanded. I now hooked my left forefinger under the coracoid process, and divided the attachments of the pectoralis minor, short head of biceps and coraco-brachialis muscles. The disarticulation of the humerus was next accomplished, and this required care, as the projection of the tumour around the joint made it necessary to be very cautious. The long head of the triceps was then divided, the scapula being carried backwards and drawn from the body, so as to allow me to divide the rhomboid and other muscular attachments with a single sweep of the knife. The loss of blood was very small, for in dissecting off the cutaneous flaps, and in separating the deltoid and trapezius from the scapula, all bleeding points were secured before proceeding farther. Subsequently, from the manner in which the axillary artery was commanded, no bleeding occurred, the subscapular and other

vessels being secured before compression was relaxed. I expected to have had bleeding from the artery of the base of the scapula, but could scarcely find a bleeding point in its usual position.

Although there was so little loss of blood, the effect of shock was very obvious when the scapula was removed, as evinced by feeble pulse and sunken features. But this state speedily disappeared, and before the wound was dressed the pulse had recovered after the administration of stimulants.

After all bleeding had been arrested, I examined the wound carefully, and noticed a series of elongated bodies of a prismatic form and a milk-white colour in the position of the axillary chain of glands, and these I dissected out. (They proved on examination to be glands, but contained no proliferous cells.) The whole surface was then washed with tepid carbolized water, and the flaps of skin having been brought together by sutures, a long flat pad of lint enclosing "marine tow" was placed in the axilla and along the inside of the arm, which was then secured to the side by a broad flannel roller. The lines of incision were dressed with strips of dextrine oiled silk, wetted in carbolic lotion, and secured by gutta percha tissue glued down at the edges by touching them with chloroform.

The following extracts from the report in the Hospital Journal will best show the subsequent progress of the case:—

"February 22nd. The patient has passed a tolerably good night; pulse 120; little discharge from the wound. He is getting milk and white of egg for diet, and occasional small portions of brandy and water and ice to allay thirst.

"24th. Patient much as at last report, but troubled with severe bronchitic cough. Two stitches were removed to-day. The wound is looking very well, and there is only very slight discharge.

"25th. Patient has had less cough, but complains of dryness of his mouth. The tongue is moist and not much furred. Pulse has fallen to 108, and is of fair strength. Diet, milk with white of egg, beef-tea, and brandy. Quinine in doses of 1 gr. with dilute sulphuric acid in water to be given thrice a day.

"27. The cough has again been severe, otherwise the condition of the patient is satisfactory. The discharge is very small in quantity, and has no odour. Some stitches were removed to-day, and the greater part of the incisions corresponding to the back of scapula seem united by first intention. There is a little irritation and discharge at the wound over the section of the clavicle, and the head of the humerus has a tendency to

press against the cicatrix at the upper part, causing it to break up.

"March 1st. Patient going on favourably. Cough less. The tongue is slightly furred; pulse 108, regular, and good strength. Ordered a cathartic, as his bowels have been confined.

"4th. Patient complains of feeling weak and is very irritable, but his general condition is good. Pulse 104; temperature of skin natural. Diet, steak or chop, with sherry 4 oz., and 2 oz. of brandy.

"11th. Patient is more cheerful; is still troubled with his cough, but takes his food well. Pulse 100. All stitches have now been removed some days since; all the large wound has closed, with the exception of the upper part, where the head of the humerus still tends to project, no matter how the arm is placed, and even when he is sitting up in bed with the elbow unsupported.

"13th. Pulse 90; little or no discharge.

"23rd. Progressing favourably.

"26th. Patient has had a fresh attack of bronchitis, which has rather exhausted and dispirited him. The wound is healed, except at upper part, which is now contracting.

"April 13th. Patient allowed to get up. Wound nearly healed over head of humerus. His cough is greatly better.

"May 7th. Dismissed cured."

Remarks.—The cases in which excision of the entire scapula has been successfully performed are as yet few, and such instances as that just recorded are worthy of publication as encouragement to operate. The foregoing case has, moreover, some interest in other respects as regards the history and character of the tumour, and as drawing attention to the operative procedure which should be adopted to facilitate the after treatment of the wound.

The tumour for which excision was performed presented some peculiarities when we contrast its vital manifestations with the structural appearance and invasion of other tissues. It had orginally, and, indeed, up to a recent period, presented all the vital characters of a simple growth—very slow progress during twenty years, no pain, mobility and definition, and absence of any constitutional cachexy or disturbance; but its structure when cut into presented at one part all the appearance to the naked eye of medullary growth, and it had not only involved part of the capsule of the shoulder-joint, but had also infiltrated the neighbouring muscles and axillary glands which were removed.

These conditions were at variance with all my past observation of tumour growth. A very considerable experience had

led me to lay down as an axiom that when a tumour was defined, of slow growth and possessed of the other manifestations of simple tumours, we might safely reckon on its maintaining that benign character, however much it might alter locally—that is to say, such a tumour, though it might ulcerate and adhere to other textures in consequence of local irritation, would not invade other tissues as malignant growths do; nor would it affect the constitution with true cachexy, and might be safely removed without risk of reproduction. In this case, however, the section of the tumour, the involvement of other structures, and more especially the infiltration of the long head of the triceps and other muscles, and the neighbouring chain of glands, so impressed myself and others who saw it that, on exhibiting the tumour to the Medico-Chirurgical Society, I expressed myself as rather shaken as to the views I had long held by what seemed so marked an exception to what I had hitherto observed. Subsequent microscopic investigations, however, would seem rather to confirm the axiom I have long held, for neither in the medullary-looking part of the tumour nor in the altered axillary glands was there any appearance of cancer cells.

The trouble experienced in the after treatment of the wound in this case has led me to reconsider the method of operating, and whether that might not be modified with advantage. The troublesome part of the treatment was the difficulty of obviating the constant tendency of the head of the humerus to be protruded at the upper part of the wound. Whilst the large posterior lines of incision corresponding to the body of the scupula healed almost entirely by the first intention, that part of the wound which corresponded to the outer end of the clavicle and acromion process was prevented from healing, and the cure rendered tedious by the pressure of the head of the humerus, and also to some extent by the irritation caused by the edges of the section of the clavicle, and when this part of the wound did heal, the head of the humerus was adherent to the cicatrix.

In the special memoir of a successful case of excision of the scapula published by the late Professor Syme no such difficulty is mentioned, but the reason is obvious. In Mr. Syme's case the head of the humerus had been excised some time previously on account of disease, and hence both the operation and after treatment of the wound would be simplified.

At first sight it may seem that there would be little difficulty in obviating the protrusion of the head of the humerus. That by merely attending to position, keeping the arm at a right angle with the trunk, or elevating it so as to depress the head of the humerus, the object in view would be attained. Such

was my own opinion at first, but when I tried to put these plans into practice I found that they did not answer my expectations. When the arch formed by the acromion and clavicle is removed and the scapula excised, the cavity of the axilla ceases to exist, and the head of the humerus, however placed, is close to the surface, and has a tendency to be drawn or pushed upwards by the action of the pectoralis major. Moreover, it was found impossible to retain the arm in the positions indicated for any length of time. The only method that had any effect, and that very slight, was fixing the arm by bandaging it to a broad roller fastened round the pelvis.

Resection of the head of the humerus in such circumstances is a very obvious method of getting rid of the difficulty, and Mr. Syme's case is an instance in point; but unless the part was implicated by the disease, I think it would be unadvisable to add to the risks of the operation by such complication if we can attain our object otherwise.

The method I would adopt now would be to disarticulate the clavicle from the scapula, instead of sawing it through. By this means we would avoid a source of irritation produced by the section of the clavicle, and at the same time by leaving the broad flat acromial extremity of that bone we would have a *point d'appui* with which the head of the humerus could be kept in contact, and so prevented from protruding, and there a false or new joint might ultimately be formed.

Such a procedure might render the operation in some respects more difficult, because by sawing through the clavicle, as in the case narrated, the operator can most readily expose the great vessels so as to enable the assistant to command them thoroughly, and he can almost easily expose and deal with the muscles attached to the coracoid process; whereas in disarticulating the clavicle from the scapula he requires to divide both the acromio and coraco-clavicular ligaments from above and behind after separating the attachments of the trapezius, and to avoid division of the integuments below the clavicle. Practically, however, I believe the difficulty could not be great, for I have on one occasion excised the scapula by this method in a case in which, after removing a large medullary cystic tumour from the axilla, I found there was a medullary growth from the subscapular fossa. In that case, owing to the exhausted state of the patient, I had to proceed very rapidly, and on the spur of the moment I divided the muscles attached to the base of the scapula and the trapezius, threw forward the scapula and arm, divided the coraco-clavicular ligament, separated the clavicle from the acromion, and removed the scapula after disarticulating the shoulder-joint. In Dow's case I adopted what I considered the more methodical plan of

operating to secure against hemorrhage, as already mentioned; but I believe that important object could be equally attained by direct compression above the clavicle through the incision necessary to separate the trapezius muscle, whilst the extent of the wound would be diminished, its after treatment simplified, its healing expedited, and a more useful arm left.—*Dublin Journal of Medical Science, June* 1873, *p.* 508.

45.—ON EXCISION OF THE EXTREMITY OF THE HUMERUS AS A REMEDIAL MEASURE IN CASES OF ANCHYLOSIS OF THE ELBOW-JOINT RESULTING FROM INJURY.

By Dr. PATRICK HERON WATSON, F.R.S., Senior Surgeon, Royal Infirmary, Edinburgh.

In cases of injury to the elbow-joint in which anchylosis, partial or complete, has resulted, excision of the elbow-joint has long been recognised as a legitimate procedure, the movements of the limb resulting from the excision of this joint being so very much more perfect than those effected when either complete or partial anchylosis impairs the flexibility of the elbow.

The resulting limb, however, in cases of excision for anchylosis, has not, in my experience, been so satisfactory as in those cases where disease has been the occasion of operative interference. On the one hand, too great a degree of mobility in every direction has been the result; on the other, the union between the divided ends of the bones has been more complete than could be desired, and the movements have been commensurately imperfect. In one case, when the latter condition resulted, I practised resection no less than three times, cutting away the osseous textures freely, but on each occasion ossific growth rendered the movements of the limb very unsatisfactory. In the former class of cases (where too great mobility has been the fault), a want of muscular development has apparently formed the chief obstacle to the usefulness of the limb, and the want of controlling power of the *triceps* in extension has appeared to be what was mainly deficient.

These observations had forced themselves upon my attention for a considerable time, during which I had continued content with the simple operation of resection of the articular surfaces of the humerus, radius, and ulna. The first modification I attempted with a view to saving an insertion of the *triceps*, consisted in performing, in the summer of 1869, in a case of fracture of the end of the humerus, a subperiosteal resection of the elbow-joint. This was effected by a straight or linear

incision through the *triceps* and over the olecranon; the insersertion of the *triceps* and the periosteal covering of the upper end of the ulna were then turned aside, and the olecranon cut off obliquely, so as to divide the coronoid process as well, and thus detach both the greater and lesser sigmoid cavities of the ulna. The end of the humerus was next cut off by the saw, an excision director having been carried round its anterior aspect, in the groove of which the key-hole saw had free play. These portions of bone having been extracted through the wound by means of sequestrum forceps, the head of the radius was clipt off by the bone-pliers. The result of this case was not satisfactory, extensive osseous deposit rendering the movements less perfect than was anticipated. I repeated this method, however, upon two occasions., but in both there was too little movement to satisfy every requirement.

In the summer of 1871, I operated upon a boy by a new method, which I conceived would fulfil every indication, so far as the preserving muscular attachments was concerned, and at the same time enable me to effect the removal of as much of the osseous textures as might appear to be necessary. The speculative reasonings which led to my adoption of this method were these:—

It was quite obvious that, as in most cases of fracture into the elbow-joint, the humerus was the bone alone affected; no changes in the osseous structures of the radius and ulna necessarily resulted from any injury the humerus had sustained; nor even should the radius and ulna be involved in the injury, did the resulting efforts at repair constitute a condition which implied any need for their removal by operation. It was also obvious that the removal of the upper extremity of the ulna necessarily impaired the perfection of the muscular attachments, viz., of the *triceps* and the *brachialis anticus*, and indirectly the power of the *biceps* in flexing the forearm. It was clearly, therefore, very desirable that neither the radius nor ulna should be interfered with, if removal of the extremity of the humerus alone would suffice to remedy the anchylosis.

The operation I devised for carrying out these theoretical requirements consisted in the following steps:—(1.) A linear incision to be made over the ulnar nerve to the inner side of the olecranon process rather longer than that usually employed in the ordinary excision of the elbow by linear excision. (2.) The ulnar nerve to be turned over the inner condyle by careful dissection. (3.) A probe-pointed bistoury to be introduced into the elbow-joint in front of the humerus, and then behind that bone, and carried upwards, so as to divide the upper capsular attachments in front and behind. (4.) A pair

of bone-forceps to be next employed to cut off the entire inner condyle and trochlea of the humerus, and then introduced in the opposite diagonal direction, so as to detach the external condyle and capitulum of the humerus from the shaft. (5.) The truncated and angular end of the humerus to be cleared, turned out through the incision, and smoothed across at right angles to the line of the shaft by means of the saw, whereby (6.) room might be afforded, so that partly by twisting, partly by dissection, the external condyle and capitulum are removed without any division of the cutaneous tissues on the outer side of the arm.

This operation, it will be observed, by the single linear incision upon the inner side of the arm, enables the operator to drain efficiently the entire area of operation, and through an incision of very moderate limits to remove the entire expanded extremity of the humerus, without interfering with any muscular structures, except those of the forearm, which take origin from the osseous tissues actually excised. The result in this instance was perfectly satisfactory, the movements of the forearm being restored so as to maintain a degree of muscular power not usually observed in cases of ordinary excision of the elbow.

Shortly after this, in August 1871, I operated in precisely the same manner upon a young gentleman suffering from anchylosis of the elbow-joint, consequent upon a fracture of this joint. Here not only had the humerus suffered, but in the course of the operation the coronoid process of the ulna was seen to have been broken off, leaving as a result a line of fibrous union traversing the greater and lesser sigmoid cavities of the head of the ulna.

All went on well in this case, till the wound had almost healed, when chronic osteomyelitis of the humerus set in, and a rigidity began again to return, which resisted diligently-maintained passive motion effected under the influence of chloroform. In these circumstances I again opened up the line of incision, and found that a long spike of new bone, developed from the granulation tissue, which projected from the cancelli of the centre of the divided surface of the humerus, had become united to the surface of the ulna. This projection, along with half an inch more of the shaft of humerus, I then cut off.

After this the wound healed, and the movements of the forearm became steadily restored.

In the same week I operated upon a lad sent to my care from Caithness-shire, who had met a severe injury to the elbow-joint by falling from a roof. I practised the same

operation I have described, and had the satisfaction of obtaining a most perfect result.

I have since then operated upon two female patients and a child in precisely the same manner and with equally satisfactory results.

I have thus practised this mode of operation in six cases in all. With a single exception, a satisfactory result accrued immediately from the operation. In this single case, when an attack of osteomyelitis supervened upon the operation, and osseous union was threatened between the humerus and bones of the forearm, the secondary removal of a further slice of the humerus afforded an ultimately satisfactory issue.

The merits of this operation, which, so far as my observation and reading go, is an original one, consist—(1.) In leaving the attachments of the *triceps* and *brachialis* undisturbed, affording therefore a degree of leverage in the movements of the forearm which cannot be attained when the olecranon, or any portion of the upper end of the ulna, are interfered with or removed. (2.) In limiting the area of operation almost exclusively to within the capsular ligament of the elbow-joint, which seems to secure more speedy healing of the wound than would otherwise occur. (3.) In securing, by the line of incision being internal and posterior, less ultimate surface deformity, a more direct drain for discharge, and a more ready access to the ulnar nerve than by any other method.

One objection only can be taken to this mode of procedure, viz., that it does not afford a ready access to the external lateral ligament of the elbow-joint; this, however, is of trivial importance, if the plan of procedure I have laid down be rigorously carried out in the division and removal of the end of the humerus, viz., 3. The oblique division of the condyles of the humerus from above downwards, so as to cut through the articular surface by means of bone-pliers between the trochlea and capitulum of the humerus. 2. To cut off the capitulum and external condyle obliquely from the shaft by means of pliers applied from below upwards. 3. To turn out the end of the shaft and cut off as much of its truncated and conical extremity as may be deemed requisite; and, lastly, to dissect and twist away the capitulum and external condyle from their remaining ligamentous and other attachments.

It may be urged, that while this may be easy enough when there is only partial regidity of the elbow-joint, it is impossible to effect it in cases of complete and absolute anchylosis of the elbow-joint. But such an objector must not fail to recollect, that absolute anchylosis of the osseous kind is not a common result of fracture into the articulation, especially when passive motion has been attempted to be kept up after the

accident; that in most of these cases it is rather due to the altered form of the osseous surfaces resulting from the fracture and displacement, and that at most the anchylosis is usually fibrous in its character.

Again, even were it present, forcible flexion and extension under chloroform will, in the great majority of cases, effect such a degree of solution of continuity as will enable the operation to be carried out in the manner already described without any real difficulty. Should any case occur of very dense osseous union of the articular surfaces, rendering the risk of fracture of the olecranon or of the shaft of the humerus a reasonable danger possibly involved in such strenuous effort, then a transverse section of the humerus with bone-pliers through the condyles, excision of a portion of bone above this level, and piecemeal excision of the anchylosed condyles themselves, by means of the forceps and gouge, would afford an alternative means calculated to remove any ordinary difficulties; while the conversion of the operation into a complete excision of the elbow-joint may always be had recourse to should insuperable obstacles be found to prevent the execution of the more limited resection.—*Edin. Medical Journal, May* 1873, *p.* 985.

46.—ON THE TREATMENT OF LUXATIONS BY ROBERT'S MODIFICATION OF JARVIS' ADJUSTER.

By William Stokes, Esq., Professor of Surgery, Royal College of Surgeons, Surgeon to Richmond Hospital, Dublin.

[Mr. Stokes first witnessed the use of Jarvis' adjuster in the clinique of Professor Nélaton, and from the results he saw, formed a high opinion of it. A short time subsequently, a modification, and in many respects an improvement on the original adjuster, was devised by M. Robert, of Paris, and it is to this appliance that attention is especially called in the following paper.]

In overcoming the muscular resistances which oppose the reduction of dislocations, the surgeon has, as a rule, hitherto employed one or other of two kinds of force, viz., either physical or mechanical. The former has often succeeded when the latter has failed, but it frequently fails from the surgeon and his assistants being unable by it to use a sufficient amount of force. The mechanical, on the other hand, I am persuaded, often fails from it been applied between two points which, during the attempt at reduction, remain fixed. It appears to me that the great value of the instrument, to which I wish to draw attention, arises from the fact that it combines the

advantages of both the physical and mechanical methods of reduction, for while the direction of extension and counter-extension can be altered at will, it furnishes an extending force which is unlimited in principle. The indications, as mentioned by Jarvis, which this instrument is intended to fulfil are—

1. To establish a line of extension and counter-extension between any two described points, and to be enabled to change the direction of the force without any previous relaxation of it.

2. To furnish an extending force which is unlimited in principle, yet easily calculated, and which is perfectly subject to the will of the operator.

3. To be enabled to estimate with mathematical accuracy the amount of force used.

4. To enable the operator to apply the force, whether direct or transverse, either rapidly or slowly, and to retain it on the limb during his pleasure.

That a violation of mechanical principles in the application of force in the treatment of dislocations frequently occurs, and that this leads often to failure and sometimes to grave accidents, which seriously complicate the case, there can be no question, and one prime reason of the frequent occurrence of these failures and accidents has been to a great extent, as Jarvis has rightly shown, owing to the employment of forces which operate from fixed points over which the surgeon has no control. "Thus," he remarks, "if the line of extension happens to be the correct one, he (the surgeon) does not desire it to be changed, but if it happens to be incorrect the force must first be relaxed before he can change it." This difficulty, which the surgeon has often to contend with in attempting the reduction of dislocations, is chiefly evident in cases where the luxated extremity of the bone is driven into muscular tissues which tightly envelope or grasp it. Many illustrations of this might be adduced. I may mention one instance by Jarvis. He observes:—"Suppose the case where the head of the bone is driven into the soft tissues and the neck is closely embraced by them, as, for instance, in some cases of the head of the os humeri, we must first disengage the head before we can even hope to succeed in reduction; suppose, also, we apply the pulleys; force is steadily and long continued, but owing to the firmness with which the neck is embraced, and the strength with which the muscles contract it is not relieved from its confinement. Is it not plain, therefore, that should the extending point now be subject to the will of the surgeon to move at pleasure, while the force is yet constant and steady, to give free motion to the limb would greatly aid him in reduction and that without increasing the power!"

Many other illustrations I might also mention, as, for

example, in certain luxations of the femur where the head and neck are forced obliquely into the muscular tissues, which, tightly embracing the neck of the bone, must, most probably, have to be torn through and, in truth, often are in attempting to replace the bone in its normal position.

The apparatus, which I think goes far in getting rid of these difficulties and accidents, consists of a steel case of the dimensions of an ordinary gun-barrel, open at the upper surface and containing a rack-bar. At one extremity of the steel case the pinion wheel is fixed. The rack-bar constitutes the extending bar. The counter-extending-bar is fixed, not at the extremity of the rack-bar case, as in Jarvis' adjuster, but to the thigh fork, the shoulder collar, or the elbow fork. The shaft of the pinion wheel terminates in a square heel which fits into a corresponding square opening at the extremity of the lever; and which passes through the centre of a ratchet wheel which is fixed to one extremity of the lever. The rack-bar case is so adapted as to fit into the end of the counter-extending-bar, and the two bars, the extending bar and the counter-extending-bar, are united by the dynamometer which is fixed by two hooks and which are close to the extremities of the two bars. The effect of turning the pinion wheel by the lever, is to increase the distance between the extending or rack-bar, and the counter-extending-bar. This latter is attached, for luxations of the upper extremity of the humerus, to the shoulder-collar; for those of the elbow, to the elbow-fork; and for luxations of the femur, to the thigh-fork. These appliances can also be adapted to luxations occurring at the knee and other articulations.—*Dublin Journal of Medical Science, August* 1, 1873, *p.* 89.

47.—DISLOCATION OF HUMERUS AT SHOULDER-JOINT.

By Dr. CHARLES KIDD.

In dislocation of the shoulder, chloroform is of vast service: thus cases of forward and inward dislocation, old cases especially, will fail with pulleys; but by elevating the elbow to a level with the shoulder, rotating the arm, and then by the little manœuvre of drawing it back, so well known now under chloroform, the bone goes in at once. A late house-surgeon at University College Hospital relates nine such cases. Parker, of Liverpool, cites several cases also (1872), the glenoid fossa being, he thinks, the support for the neck of the humerus, but the joint is almost always set right under the "manipulation" plan.

The old methods of reducing old dislocations appear to us now very mischievous and erroneous, if not cruel. Practically,

there are perhaps but two dislocations of the humerus at the shoulder that get mixed up with our chloroform exercises: the one, forwards and inwards under the pectoralis; and the more familiar and frequent, accident, downwards into the axilla, the head of the humerus here pressing against the inferior costa of the scapula; between these (always uninviting structures in chloroform administration) large tendons, viz., those of triceps and subscapularis; for we hold injury of tendons a more frequent cause of death by chloroform than heart disease.

Need we remind the reader of these large tendons or the torn capsule; the glenoid cavity, so shallow; the stretched muscles; the difficulty, especially in old cases, of stirring the fixed head of the bone. The student will be pleasantly told these dislocations of humerus form at least half of all dislocations put together; and of these, half again are sub-glenoid, the rest subcoracoid or forwards, and about 5 per cent. sub-spinous. We pass over the immediate or diagnostic marks of the several lesions, confining present remarks to the manipulations that now so often go together with chloroform; nor need we stop to specify the three dislocation symptoms: want of roundness in the shoulder, hollow under acromion, and acromial projection, that every industrious student in the out-patient's department is aware of.

Some no doubt will think us over tedious as to symptoms, or injured tendons; the severity of the symptoms, as once explained in a college examination, being like measles in proportion to the attention bestowed on them; but it is not so.

Chloroform has systematised what Mr. Abernethy used to style "stratagem," or taking the patient, or rather his muscles, unawares, as in that way the muscles were relaxed. Now the surgeon places the dislocation patient deeply under chloroform (it must not be nitrous oxide, under which the muscles are not relaxed), then with one hand grasping the shoulder, the flexed elbow with the other (when the dislocation is sub-glenoid or subcoracoid), the thumb is next placed over the head of the errant bone, and fingers over the spine of the scapula, the thumb acting as a fulcrum; so much for one hand. Then, with the other hand, the painfully flexed elbow should be drawn from the side, extension made; some slight rotatory movement outwards also employed. The surgeon's anatomical knowledge and *tactus eruditus* will here help him much; when extension has been thus carried to its full extent, the elbow is then raised, the arm made to describe a sort of semicircle towards the sternum; then suddenly brought down to the side, the head of the bone rotated inwards, the thumb giving the right direction to the latter, and all comes right.

Without this accuracy of symptoms and anatomical knowledge

and attention in this good rule of thumb way, the surgeon will not succeed, but with it, the cure is little short of miraculous; there are modifications, such as pressing the knee into the axilla; a jack towel with clove-hitch knot on the patient's arm, two or three assistants making steady extension horizontally outwards; but we doubt if any plan is so perfect as this new method, almost the reverse of the older one. The plan of White, of drawing the arm vertically upwards is another. It has succeeded where the manipulation has seemed to fail, but both are worthy of a trial in the more puzzling cases.

In these old dislocations of humerus as well as in hip cases, there was formerly, what with pulleys, torn capsules, bandages round the arm to secure a good pull; what with clove-hitch knots, jack towels round the chest, a sort of daring or engaging barbarism that much excited the earlier students; all is now changed, ropes and pulleys and pull all together are gone; "a comic reading these as the ecraseur chain to a breast tumour," quietly observed Sir W. Lawrence to us once, "these triple folds and brass round the chest.

Aës triplex,
Circa pectus erat qui fragilem truci, &c.

A comic reading of the hazard to joints and capsules, and hardiness, and shipwreck made of some cases; recalling the ship in Horace, and the first mariner with his pulleys, and sea sickness."

Pulleys, indeed, amongst all right-thinking surgeons have given way to anæsthetics; in recent dislocation, perhaps, neither is required; but in old ones, adhesions should be broken through or made to yield by forcible rotation and flexion, rather than torn and dragged by pulleys; or these triple girths or bandages, or dragging the arm like a pump-handle vertically upwards. We are told now of such cases, a year old, quite cured by chloroform.

There are many cases of old unreduced dislocation, such as that of the astragalus, patella, &c., which also readily yield under chloroform.

Who shall say, then, that not only in reducing the agony and shock of such operations, but facilitating the cure, chloroform is not a blessing.—*Students' Journal and Hospital Gazette, Oct. 25, 1873, p. 324.*

48.—TWO CASES OF DISLOCATION OF THE HIP TREATED BY THE AMERICAN METHOD OF MANIPULATION.

By Dr. HECTOR C. CAMERON, Glasgow.

In a former series of this Journal (July 1866), I related a case, in which I reduced a recent dislocation of the hip into

the sciatic notch by means of the American method of manipulation. I have since met with the two following cases:—

1. *Dislocation on to dorsum ilii of seventeen days' standing. Reduction at first attempt.*—William Lynn, æt. 16, miner, was driving a horse harnessed to some "hutches," in a coal pit, on the 5th September, 1867. The horse ran off, and he, holding on to the reins, was drawn along by it. Being at last brought on to his knees, the hutches came up behind and struck him on the lower part of the back. When he rose he was unable to stand, experienced pain in the left hip-joint, and his "legs clung together the same as if they had been tied with a rope." About two hours after the accident he was seen by a medical man, who recognized a dislocation, and, having produced extension by pulling on the leg, while he kept one of his feet pressed against the perineum, he stated, according to the patient's account, that he believed he had succeeded in effecting reduction; explaining, that the shortening and inversion, which continued to be a source of alarm to the patient and his friends, would in all probability pass off. Thirteen or fourteen days after the accident, the patient still suffering great pain, the doctor returned with a friend. Having administered chloroform, and got the assistance of "two labourers who were working on the road," they produced violent and long-continued extension on the limb, while steady counter-extension was maintained on the axillæ. They left after many attempts, and without succeeding in their object. After a day or two of great pain, he was admitted into the Royal Infirmary. I saw him after the house-surgeon in charge had made an unsuccessful attempt at reduction under chloroform. The head of the femur was on the dorsum ilii, and all the symptoms occurring under such circumstances were clearly marked. Chloroform having been fully administered, I flexed the leg on the thigh, and the thigh on the abdomen, abducted the limb fully, and rotated outwards. During this last movement, I felt the tearing through of adhesions, and the head of the bone at once returned to the acetabulum. On extending the limb, it was seen to be in good position. The patient made a good recovery, and went to the Convalescent Home three weeks afterwards.

2. *Case of recent dislocation of the hip into the thyroid foramen. Reduction.*—Thomas Yuill, æt. 42, quarryman, was admitted on the 25th April last into a ward of the Royal Infirmary, of which I was in temporary charge.

He had been struck a short time previously by the "jib of a crane," but could not give a very accurate account of the accident. He complained of great pain in his right hip and thigh,

and a very slight examination served to show that the head of the femur was in the obturator foramen. There was slight lengthening and marked abduction of the limb; while the knee was slightly flexed, and the prominence of the trochanter was conspicuous by its absence. There was a distinct fulness in the obturator region, although, as the patient was a very muscular subject, the head of the bone could not be defined by the hand. Reduction was effected under chloroform in the following manner:—I flexed the limb as thoroughly as possible, slightly abducted, and then rotated inwards firmly, at the same time adducting the limb and bringing it towards the floor. This resulted in transferring the head of the bone from the thyroid foramen—not to the acetabulum, but to the sacro-sciatic notch. Flexion, abduction and rotation outwards (which so readily transfers the head of the bone usually to the acetabulum in the case of dislocations upwards and backwards), sent it back to the thyroid foramen. A careful repetition of the first set of movements put it again in the sciatic notch; and again as before, in trying to reduce it from that position, I sent it back to the thyroid foramen. Having again removed it to the sciatic notch, I put my foot in the perineum, and made extension aid my movements, when it slipped at once into the acetabulum. The time occupied was very short, for the head of the bone was changed from the one position to the other with provoking facility. The patient made a good recovery.

The first of these cases illustrates the ease with which a dislocation of the hip may be reduced when extension, fairly and intelligently tried both with and without chloroform, has failed to attain the desired result.

The second case is interesting, chiefly as pointing out a mishap which seems apt to occur in manipulating the thyroid dislocation; namely the removal of the head of the bone not into the acetabulum, but upwards and backwards into the great ischiatic notch or on to the dorsum ilii. In a paper by Dr. Thos. M. Markoe on "Reduction of dislocated femur by manipulation," in the New York Journal of Medicine for January 1855, this is well shown in several of the cases related. In Case 4 the original dislocation was into the sciatic notch, and Dr. Markoe says: "The effect of the first attempt was to throw the head on to the obturator foramen, making the limb longer than the other, and producing the deformity characteristic of that dislocation. From this point, by a slight alteration of the movement, the head could be made to slip back to its original position. Between these two points it could be made to play backwards and forwards, but would not enter its socket." Much the same thing is described in several other cases. In all the cases of thyroid dislocation, it would seem that extreme flexion was employed and rotation was found to succeed,

whether made outwards or inwards. It seems as if too great flexion is the cause of the bone slipping from the one position to the other without entering the acetabulum. At all events, this is the opinion expressed by Bigelow and Hamilton, and in the case related above I flexed the limb to the fullest possible extent.

Bigelow writes in regard to Dr. Markoe's cases of thyroid dislocation: "These cases are instructive as showing that the head of the bone is directed towards the socket when the Y ligament is wound upon the shaft by rotation, *whether inward or outward*, and they correspond to the results of my own experiments, made before I had read them. . . . . It may be superfluous to add, that in Markoe's case, inward rotation would probably have reduced the bone, had the thigh been less flexed." Hamilton says: "In one example mentioned by Markoe, it is pretty evident that the head of the femur was thrown into the ischiatic notch, by having flexed the thigh too much, so that 'the knee touched the thorax.' Indeed, it is questionable whether it will be best ever to bring the thigh much, if at all, above a right angle with the body, since any further flexion can only throw the head below the acetabulum, when, in fact, it is already too low." In backward dislocations, on the other hand, the flexion should be firm and complete.—*Glasgow Medical Journal, August* 1873, *p.* 444.

49.—ON PROFESSOR ESMARCH'S MODE OF PERFORMING BLOODLESS OPERATIONS.

By Wm. Maccormac, Esq., Surgeon to St. Thomas's Hospital.

The importance of the simple and efficient method briefly described by Professor Esmarch at the second Surgical Congress in Berlin, for preventing loss of blood during operations on the limbs, appears to me so great, that I take the earliest opportunity of communicating my experience on the subject, brief though it be.

A little girl, 5 years old, struck the left tibia, twelve months ago, against a stone; necrosis followed, and when admitted to Hospital a year afterwards a sequestrum could be felt in the tibia enclosed by a considerable thickness of new bone. Whilst the patient was being chloroformed, I applied, pretty tightly, an ordinary elastic bandage from the toes to the middle of the thigh. The bandage was two inches wide and five yards in length, and, thus applied, the bandage forced all, or nearly all, the blood from the limb into the body. When the patient was fully narcotised, I wound a half-inch indiarubber rope around the thigh immediately at the upper border of the band-

age, and sufficiently tightly to obstruct all the afferent vessels. Hooks previously attached to the extremities of the rope furnished a ready means of fastening it, as well as of removing it at pleasure. The bandage first applied was now unrolled, when the limb presented a blanched appearance. The operation was then commenced; some new bone removed, so as to get at and take away a considerable-sized sequestrum. During the entire time not a single drop of blood appeared in the wound; a sponge was not once required, and the facility with which the operation was conducted and finished requires to be seen to be realised. The tissues were divided, so far as bleeding was concerned, just as they might have been on the dead body. This operation was performed in St. Thomas's Hospital on August 16 in the present year, Esmarch's method for producing local anæmia being then practised, I believe, for the first time in Britain. Since the operation the little patient has progressed very favourably, and, although carefully watched, no peculiarity which might be attributed to the use of the apparatus has been observed either in the wound or in the limb.

Since then other operations for necrosis have been performed, and an excision of the knee lasting thirty-five minutes, also an amputation of the thigh, and in no instance has one single drop of blood been lost. The advantages of such a plan are so palpable as not to need much insisting upon. The generality of hospital patients can ill spare a serious loss of blood, and such a loss often proves inevitable during operations for extensive necrosis of bone. In amputations the greater part of the blood of the lost extremity is preserved, to the advantage of the patient. The duration of operations will be much shortened, as there is neither blood nor the constant dabbing of sponges into the wound to remove it, to interfere with the surgeon's sight, No accident or ill consequence at all appears to follow the use of the apparatus. In cases where amputations require to be performed for gangrene, or where there is a deposit of septic material in the limb about to be operated upon, there might be a risk of the elastic bandage forcing some portion of the septic material into the circulation. In the further use of the apparatus this possibility must be kept in view. Anyone will be surprised, in trying it upon his own arm, to find what a small amount of pressure of the india-rubber rope will stop the pulsation of the radial artery, and the femoral can also be stopped with no great exercise of force. Doubtless the history of surgery abounds with many attempts to empty limbs of blood previous to amputation, and to arrest hemorrhage during their performance. Stromeyer, in 1853, as he relates in his "Maxims," adopted a plan precisely

similar in principle, in an operation on a brachial aneurism. He bandaged the limb to a point just above the aneurism, and then applied a tourniquet. The loss of blood was very small during the operation. Billroth mentions that when he was assistant to Von Langenbeck in 1853 and 1854 a somewhat similar plan was tried in the clinique in Berlin. Vanzetti, of Padua, relates in the Italian Medical Gazette that Dr. Silvestri, in Vicenza, has employed bandaging and the indiarubber rope compression above it in amputations; but, this notwithstanding, to Professor Esmarch must be attributed the credit of devising and making known a most simple, practicable, and efficient plan for wholly preventing loss of blood during operations, of whatever kind, when performed upon the extremities of the body.—*Medical Times and Gazette, Sept.* 20, 1873, *p.* 320.

50.—MODIFICATION OF ESMARCH'S ELASTIC BANDAGE.

By W. H. CRIPPS, Esq., House-Surgeon to St. Bartholomew's Hospital.

Esmarch's admirable suggestion of using an elastic bandage to exclude the blood before operating on limbs, and the complete success attending it, are now probably well known. The following is a simple modification of his arrangement, by which many yards of elastic bandage may be dispensed with, and it can be easily and quickly applied.

A short india-rubber tube is used, not only to prevent the blood from returning to the limb, but also for the purpose of removing it in the first place. The two ends of an india-rubber tube, twenty-one inches in length and about three-eighths of an inch thick, are bound together with a piece of twine, the whole forming an elastic ring seven inches in diameter. A grooved reel revolving between a double handle completes the necessary apparatus.

To apply this to the arm, three or four complete turns of the elastic ring are wound tightly round the hand in such a manner as to include the fingers and thumb, care being taken that the turns lie even and do not cross one another. The reel is then put under the free portion of the ring connecting the upper and lower coil. The reel is passed round and round the limb in an upward direction: thus each coil is unwound from below as another is added above. In this way four tight coils of india-rubber are carried up the limb to any distance required. The degree of tightness can be regulated with the greatest nicety by the distance the reel is drawn from the limb by the bandager.

This method of driving blood from the limb answers perfectly in the arm and in the lower part of the leg; but in carrying the bandage over the popliteal space the flexor tendons prevent the artery being effectually compressed. A firm pad in the space would probably answer the purpose.

To remove the bandage, it may either be unrolled by reversing the action of the reel, or the twine connecting its two ends may be cut with scissors.

A very neat little reel in a holder may be obtained at Ferguson's, in Giltspur-street.—*Lancet, Oct.* 11, 1873, *p.* 520.

51.—ON THE ANTISEPTIC SYSTEM, AS SEEN IN PROFESSOR LISTER'S WARDS AT EDINBURGH.

By RICKMAN J. GODLEE, Esq., Surgical Registrar at University College Hospital.

Although the vital importance in surgery of the antiseptic system is now very generally recognised, it is difficult for those who have had little experience of the advantages to be derived from the treatment to form a just appreciation of its merits, even though fully convinced of the truth of the great principles upon which it is based. To such, therefore, an account of the comparatively recent improvements in practical details may not be without interest, illustrated by notes of a few cases, showing the character of the results which are now obtained in Mr. Lister's wards at the Royal Infirmary of Edinburgh; and as I have during three months of last session enjoyed unusual opportunities for observing for myself the efficiency of the treatment in his hands, I have asked and obtained his consent to the publication of the following facts, some of which have not hitherto been laid before the profession.

Mr. Lister principally relies at the present time on three antiseptic agents: Carbolic acid. boracic acid, and chloride of zinc, each of which is possessed of peculiar properties, which render their employment advisable under different circumstances. The first differs from the other two in being volatile, and is therefore employed by preference in every case in which a cavity exists, into which regurgitation of air is liable to occur, both during and after the changing of the dressing; any such air is deprived of its septic influence by the vapour of carbolic acid which exists between the meshes of the gauze or amongst the drops of the spray; but it would be quite unaltered after passing through a substance containing a non-volatile antiseptic salt, such as boracic acid, the action of the latter being confined to the objects with which it comes into

actual contact. The proportion of the ingredients with which the gauze is saturated is as follows: by weight—carbolic acid, 1; resin, 5; paraffin, 7. The resin is employed on account of the tenacity with which it holds the carbolic acid, the paraffin (which gives it up with great readiness) being added to give the mixture a suitable consistence. In making a dressing a sufficiently large piece is folded in eight layers to overlap the wound in all directions, to a greater or less extent, in proportion to the amount of discharge that is anticipated. Between the two outer layers a piece of thin macintosh cloth (stained red for distinction) is interposed, by which the discharge is made to traverse the whole of the seven inner layers before it becomes exposed to the causes of putrefaction by reaching the surface. Without such an arrangement the continual flow of fluid would soon exhaust the carbolic acid from the part of the gauze immediately over the wound, and this would then cease to form a barrier to the spread of decomposition inwards.

For use as a lotion, carbolic acid is dissolved in water in the proportions of 1 part to 20, or 1 to 40; the weaker solutions being now abandoned, as they did not appear to be perfectly trustworthy. The 1 to 20 lotion is employed for destroying already existing organisms, as,—*e.g.*, in the injection of a recent compound fracture, for purifying the epidermis of a part on which operation is to be performed, or for the cleansing of an ulcer on which putrefaction has been allowed to take place. The solution of 1 in 40 of water is used for the spray, and during the changing of an ordinary dressing, and for most purposes it will be found a lotion of very convenient strength. Before an operation, some of the instruments, such as the saw and cutting pliers, are purified by smearing them with a solution of one part of the acid in ten of olive or other sweet oil, and the same is used as a temporary dressing after operation on some putrid cases to be afterwards described (*vide* the case of W. McK.); but for cleansing knives, and most other instruments, for which the oil has no special virtues, either in facilitating their working or in penetrating intricacies of their surface, dipping in 1 to 40 watery solution is quite sufficient. It is of great importance that catheters, &c., should be freed from septic germs before use, as there can be no doubt that in very many cases decomposition of urine has followed the introduction of instruments into the bladder. For this purpose a 1 to 50 of oil is found to be sufficiently powerful, and it does not, as do stronger solutions, prove in the least irritating to the urethra.

The antiseptic qualities of boracic acid were discovered a short time ago in Sweden; but though used for preserving articles of food, it had not proved of any value as a surgical

application until its slight solubility in water was ingeniously turned to account by Mr. Lister, for the purpose of storing up a supply of the acid in the so-called boric lint. As boracic acid is much more soluble in hot than in cold water, the effect of soaking lint in a saturated boiling solution is a copious deposit of the crystals throughout its substance on cooling and drying; and as these will be but slowly acted upon by the discharges from the wound, which are at or below the temperature of the body, the lint remains for a considerable time efficacious as an antiseptic application. A cold saturated solution of the acid is also employed as a lotion, with the assistance of which a moist dressing may, if required, be made by wetting with it the boric lint, over which is placed a piece of ordinary oiled silk or gutta-percha tissue. But, as a general rule, in cases free from putrefaction it is used as a dry dressing, overlapping a piece of the protective moistened with the lotion, and it is often found very convenient to fix the lint by means of collodion, its edges having been previously frayed out with a pin for the purpose. The special advantage of boracic acid is its extreme blandness; it is in substance almost tasteless, and as the crystals themselves are very soft and almost greasy, resembling closely in physical properties those of the crystallisable fats, and it acts but very slightly either as a mechanical or chemical irritant; but its non-volatility, for the reasons before mentioned, limits its application to superficial sores, in the treatment of which it will be found invaluable.

Chloride of zinc, though long known as a surgical remedy, owes its reputation largely to its employment by Mr. Campbell de Morgan, of the Middlesex Hospital. It is remarkable, as well for the potency of its action as for the duration of its effects; thus, besides being a most efficient corrector of putrefaction, it has the advantage of preserving or pickling the part to which it is applied for about three days. But as, like boracic acid, it is non-volatile, and is moreover a powerful caustic to the tissues, its employment is confined in Mr. Lister's practice to a limited number of cases. He makes use of a solution of 40 grains to the ounce of water for application to the surfaces of the wound, after operating on a part where decomposition is already present, or on one which communicates directly with one of the natural cavities of the body (as, for example, after removing piles or cutting a fistula); and in this way, even if putrefaction be not completely eradicated, its noxious products cannot influence the general cavity of the wound during the three days prior to granulation, when the tissues are specially prone to irritation and absorption. For the method of employment in the former class of cases, I may refer to that of J. W. given below, and to that of W. McK., who, besides other inju-

ries, had sustained a severe compound fracture of the ring-finger of the right hand, which was not submitted to Mr. Lister's inspection till several days after the accident, when it was found that the wound had putrefied and was much inflamed, having assumed a sloughy appearance, while the proximal phalanx was necrosed. Mr. Lister amputated the finger, removing the necrosed phalanx with dressing forceps, and fashioning two lateral flaps from the inflamed tissues. After removing, as far as possible, the adherent sloughs, the raw surfaces were freely treated with the solution of chloride of zinc, and a piece of lint soaked in carbolised oil (1 to 10) was inserted between the lips of the wound, which were not brought together at all, and the whole hand was loosely enveloped in the antiseptic gauze. The oiled lint was changed by the nurse every three hours during the first day, and afterwards the little and middle fingers were bandaged together and the ordinary gauze dressing applied, the case being from this time treated as though it had been "aseptic" from the first. The wound rapidly assumed a healthy appearance; while the discharge, which had previously been copiously and foully offensive, became serous, and rapidly diminished in amount, remaining free from putrefactive odour to the last. Very soon after the operation symptoms of tetanus appeared, which eventually proved fatal to the patient; but it must be added that this is only the second case of tetanus that has occurred in Mr. Lister's practice during the last six years, and, as in both of these decomposition had been allowed to take place, it is only fair to infer that in them, at least, it had something to do with the disease. It is also worthy of remark that in Mr. Lister's wards all arrangements as regards bedding, utensils for urine, &c., are so planned as to avoid as much as possible the vitiation of the atmosphere by putrid emanations, and this circumstance no doubt aids the antiseptic management of the cases in bringing about the perfect immunity from pyæmia and hospital gangrene, which, considering how crowded the beds are, constitutes one of the most remarkable as well as beneficent features of the treatment.

After operations about the face or mouth, the action of chloride of zinc is found to be exceedingly beneficial. Thus, in the case of S. F., half of whose lower jaw was removed by Mr. Lister, on January 15th, 1873, on account of a very large and rapidly-growing sarcoma, the chloride was applied freely to the surface of the wound, while over the incision in the skin a boric dressing was placed, under which union by the first intention went on without interruption. There was not the slightest offensive smell about the patient's breath on the second and third days after the operation, and he was thus

saved from the irritation, to say nothing of the risk, which is so often connected with this period in such cases.

All those who have applied a lotion containing carbolic acid to a raw surface must have noticed, as a result, that the serous discharge of the first few hours is much greater in amount than that which occurs in similar cases where simple water is used; and this evil—for an evil it is—is probably a necessary one in connexion with the antiseptic treatment, as it can hardly be supposed that an agent powerful enough to destroy the living causes of putrefaction should be absolutely unirritating to the higher animal tissues. It involves, however, the necessity for providing a free drainage at first; but as, if decomposition do not occur, no subsequent inflammation is anticipated, and as in most cases early union of the cutaneous surfaces is of very great importance, the simple method of attaining this result by leaving a wide interval between the stitches is seldom advisable. Accordingly, Mr. Lister makes use in almost all his cases of the drainage-tube, his modification of which is as follows:—A piece of india-rubber tubing, provided with suitable holes, is cut obliquely at the inner end, so that it may not be closed by pressing against the flat surface of the interior of the wound, while through opposite sides of the tubes, close to its outer extremity, two pieces of silk are passed with a fine needle, and the ends of each are cut at a distance of about two inches, and fastened together by a knot. It should be of such a length that, when inserted as far as possible into the wound, its outer end, previously cut obliquely or transversely, according to the direction of the sinus, may be exactly level with the surface of the skin, a position which the pieces of silk will be found to maintain. With such an arrangement the discharge is shed with great perfection, and the surgeon may place the stitches as close together as he pleases.

Another way of accomplishing the same thing is by means of the sponge, the use of which is indicated in those cases where a cavity remains, the walls of which it is desired to keep in apposition by the uniform pressure which this method provides. The sponge, previously soaked in a strong watery solution of carbolic acid, and thoroughly squeezed, is applied over the wound between the protective and the folded gauze, the whole dressing being firmly secured by a bandage. Unless combined with a drainage tube, a pretty wide interval must be left between the stitches. As an example of its employment, I may refer to the case of E. F., a young lady four years of age, from whom Mr. Lister removed an enchondroma of the size of a hazel-nut, growing from the posterior border of the scapula. At the operation, which took place on March 5th, 1873, the arm was held well back by an assistant, so as to render the

tumour prominent under the skin, and an incision 1½ inch to 2 inches long, was made down on to this parallel to the edge of the bone; while another, 1 inch long, was carried outwards at a right angle, from the centre of the first. After cutting through the superficial fat and the overlying muscle, an oblique notch was made in the scapula on each side of the tumour, and the piece enclosed between the two was then removed with cutting pliers. When the arm resumed its natural position, the edge of the bone was at some distance from the wound. No stitches were employed; but protective was placed on the incision, and over this the sponge, and then the gauze dressing, and the whole was well secured by a bandage. The sponge was removed on March 6th; and neither then, nor at any subsequent period, could anything be pressed out of the wound, nor was a trace of pus seen from first to last. On March 21st, a sound cicatrix, evidently some days old, was disclosed on finally removing the dressing.—*Lancet*, *May* 17, 1873, *p.* 694.

52.—THE APPLICATION OF EARTH IN SURGERY.

By Dr. Joseph Groves, B.A., Venice.

It was my good fortune during the winter to be residing in the same hotels, both at Nice and Rome, with the eminent American surgeon, Dr. Hewson, of Philadelphia, who brought to my notice the advantages attending the use of earth as a dressing in surgical cases. He told me he attributed his great success at the Pennsylvanian General Hospital—a success which has been remarkable since the American war, especially in connexion with cases of amputation—entirely to the use of this agent. The results of its use which have come more or less directly under my own observation have been so successful that I venture to send you brief outlines of the cases.

1. A case of epithelioma of the cheek, of twelve years' standing. In December there was an ulcer on the right cheek larger than a half-crown piece, which was being treated with dry-earth applications. In March I saw the case dressed in Rome, as did also Dr. Pantaleoni and other physicians. There were then, instead of the extensive ulcer of three months before, superficial points of ulceration which, had they been collected together, a fourpenny-piece would have covered. The patient, a gentleman of sixty-two years of age, was not very sanguine, having had the unfavourable opinions of several distinguished surgeons, but he expressed himself most gratefully for the relief from pain and from the horribly fetid odour, which made life a burden to him before the earth was applied.

2. A physician at Mentone, under Dr. Hughes Bennett's care I believe, sent for Dr. Hewson from Nice to resect his knee-joint, or to amputate his leg. Dr. Hewson found an abscess, thought to be metastatic, in the head of the fibula. He begged to be allowed to try earth dressing before proceeding to an operation. The case did perfectly well, and the joint is apparently as good as ever.

3. The American chaplain in Rome was thrown from his horse, and struck his leg against a rail. The trowsers were not cut, but the tibia, the spine of which was laid bare for several inches, sustained a comminuted fracture, and the tibialis anticus was partly stripped from its attachment. Dr. Hewson put in two or three stitches, and applied earth dressing. The patient had scarcely any pain; there was but little discharge, and the leg rapidly healed. The almost total absence of pain was very remarkable.

4. At Naples a poor woman accosted me in the street, and asked me to cure a large chronic ulcer on the right leg, which extended from the external malleolus to midway between the ankle and knee. I offered her a few soldi, and told her to go to the hospital. She said she was not begging for money, but to be cured, and as I was an English doctor, I could cure her if I would. Her grandfather had been in the service of an English nobleman, it seemed. I managed to find some light-yellow clay, allowed it to dry, pounded it and sifted it through fine muslin, and applied it to the sore, covering it with ordinary grocer's blue paper, the edges of which I wetted in order to make it adhere to the skin. I told the poor creature to remain at home and rest the ulcerated leg on a chair even when she sat spinning at her door. The next morning I found her waiting for me at the gate of the hotel. She said she was quite free from pain; she had slept all night, a thing she had not done for months before; and with tears in her eyes she thanked me for the relief I had given her. In three weeks the ulcer was almost well. It is right to say I took means to improve the general health.

5. An American gentleman in Rome asked me to prescribe for him, as he had gonorrhœa. The gonorrhœa was very acute, with chordæ and severe pain during micturition. As he bound me to secrecy, I could not borrow a small double-tubed catheter which Dr. Hewson uses in these cases, and was obliged to inject a large quantity of muddy water with an ordinary syringe. The patient was greatly relieved by the first application, which was repeated on the two succeeding days, though the discharge and pain had ceased on the second day. The patient had a dose of chloral and an aperient each night.

6. A young Roman, a friend of the previous patient, came

to me complaining of rheumatism. He had had a gleet eight months. I injected the urethra with muddy water every other morning for ten days, when the discharge ceased. The rheumatic pains ceased also, supporting, therefore, my friend Dr. Bond's theory of the cause of gonorrhœal rheumatism. This patient took iron and arsenic in small doses, as he was somewhat anæmic.

The freedom from pain, the destruction of all odour from the wound, the diminution of the discharges, and the rapid healing after the application of the earth, were the features common to all these cases which impressed me most.

Dr. Hewson, who will shortly visit England, believes the action of the earth mainly a chemical one, and he covers his dressing with blue paper in order that the chemical rays of light may be admitted to the wound.—*Lancet, June* 21, 1873, *p.* 894.

53.—ON THE USE OF RESIN-CLOTH, IN THE PLACE OF CERE-CLOTH, IN THE TREATMENT OF WOUNDS.

By Edward Lund, Esq., Surgeon to the Manchester Royal Infirmary.

To those who practise the system of dressing wounds advocated by Professor Lister, the use of the carbolised gauze which he employs, as far merely as expense is concerned, is a matter for consideration. This perhaps is the case more in hospital than in private practice, because in the former, at the present time, the gauze is largely used to test the value of the antiseptic method, and economy should be observed as far as possible. Moreover, beyond the cost, there is a difficulty in the preparation of the gauze, for the mixture of paraffin and acid with which the fabric is saturated must be applied to it in a heated liquid state, and, unless a certain temperature be maintained during the whole process, the gauze will be of unequal thickness and imperfectly porous. I have tried to make the gauze myself according to Professor Lister's recipe, but I have often failed to do so in these particulars. In the paper in which he first described this dressing (in the British Medical Journal of January 14, 1871, p. 30), Professor Lister refers to a very curious property possessed by carbolic acid—namely, that it can be combined with resin, and resinous matters generally, with great facility, and when so blended nearly all its irritating acrid properties are neutralised, while the compound so formed retains the power of evolving an antiseptic vapour at the temperature of the body. It is well known that to touch the mucous membrane or even the skin of the lips with a fragment of pure carbolic acid occasions pain and excoriation, in

some subjects more than in others. But when we experiment in the same way on a mixture of the acid with five parts of resin, we find all the acridity is destroyed; the acid is still present, but it is stored up and rendered harmless by this new combination.

Having verified this fact by numerous observations, I came to the conclusion that we have here a method of forming easily, without the aid of heat, an application for antiseptic purposes, in surgical practice, of great value. All that is required to carry out the idea is to saturate very thin calico gauze with such a mixture of resin and acid dissolved in methylated spirit, press it powerfully, spread it out to dry quickly so that it may become quite porous and absorbent, and it is ready for use. Yet the resin and acid thus left upon the threads of the calico after the complete evaporation of the spirit would be found too brittle and adhesive to the skin for a wound-cover and air-filter if something else were not added to impart flexibility. The substance which I employ for this purpose is castor oil, because it is the only fixed oil easily accessible which is quite miscible with spirits of wine. All samples of castor oil, by reason of adulteration, do not show this property of perfect solubility in alcohol, as stated by Pereira ("Materia Medica and Therapeutics," 1865, p. 251), and therefore I am content to use the oil if it will unite perfectly with twice its bulk of rectified spirit. The exact composition of the solution is as follows:—Carbolic acid crystals, melted, two fluid ounces; castor oil, two fluid ounces; purified resin, sixteen ounces by weight; methylated spirit, forty fluid ounces,—mix.

To dissolve these ingredients easily we must add them together in a certain order. To the resin, liquefied by heat and removed from the fire, add gradually one third part of the spirit; when these are well mixed, put in another third of the spirit, in which the oil has been previously dissolved; and lastly, the acid in the remaining portion of the spirit must be slowly added to complete the mixture. The whole must be agitated until all the constituents are thoroughly incorporated, and afterwards passed through a muslin filter to get rid of any extraneous matters. Unless this plan be adopted, the resin will concrete into a mass at the bottom of the vessel, and it will be extremely difficult afterwards to get it perfectly mixed. When thus prepared, the solution is of a dark colour, clean and free from any deposit, and it can be kept unchanged in a well-corked bottle for a long time. It is, in fact, a sort of thick varnish. To make the resin-cloth—as I term it, for the sake of distinguishing it from the cere-cloth dressing for wounds which I described in a paper read at the Leeds meeting of the British Medical Association in 1869,—it is needful

to select a very thin, cheap, porous calico, or calico muslin, known in the trade as "mull," which costs at wholesale price about 4s. per piece of twenty yards. This divided into strips, each about nine inches wide and six yards long, is reduced to a convenient form for general use. The calico should be unbleached and free from stiffening; and each of the strips should be carefully folded up, so as just to lie flatly in the press, as I am about to explain. An ordinary square tincture-press may be used to press the cloth, or such a press as is sold for copying letters, to which a tin box has been adapted, so that the plates of the press can work in it; and in this box the folded calico is placed, the solution being poured over each successive layer so as to wet perfectly every part of it. There should be an aperture at the bottom of the box, with a tap by which the superfluous fluid can be removed, collected, and used a second time. The press being brought into action, the pile of calico should be squeezed as dry as possible, all the fluid drained off, the resin-cloth taken out, laid over a few lines of string in a warm room with good ventilation, and, in an hour or two, when all traces of smell of the methylated spirit have been dissipated, the cloth may be rolled up and kept in tin cases ready for use.

It is difficult for me to state the exact cost of resin-cloth made by this process, for I have not yet bought the materials for its preparation at such wholesale prices as could be paid if it were to be manufactured in large quantities; but, allowing for some slight reduction where six or eight twenty-yard pieces of calico are bought at a time, and the solution made by the gallon, I find it comes to a fraction less than 3d. per yard of average width of forty-four inches. In using it as a dressing for wounds, I deal with it precisely as I would do with Professor Lister's antiseptic gauze, for which it must be taken as a cheap and ready substitute. I generally apply ten folds of it over the face of a wound (as in an amputation), and perhaps six folds higher up the limb for some distance, and I cover it with the mackintosh hat-lining, so as to distribute the serous discharges through the breadth of the resin-cloth thus covered. I have never found it to irritate the skin in any degree beyond what the oil-silk protective, liberally used, could control, except once, when, in the hurry of preparing the resin-cloth, I had neglected to dry it thoroughly, and it was applied still moist with the methylated spirit; the naphtha in it seeming to be the chief cause of the skin irritation. But if this precaution be observed I believe this resin-cloth will be found a very useful addition to our means of treating wounds and abscesses on antiseptic principles.—*Medical Times and Gazette, July* 19, 1873, *p.* 62.

ORGANS OF CIRCULATION.

54.—ON ANEURISM OF THE CAROTID ARTERY.

By TIMOTHY HOLMES, Esq., M.A., Professor of Surgery and Pathology to the Royal College of Surgeons.

[There are three modes of treating carotid aneurism, each suitable to different cases; they are,—proximal pressure, distal pressure, and the Hunterian operation of ligature of the artery. The pressure may be digital or instrumental.]

The more of healthy artery there is accessible below the tumour, the smaller the latter is, the slower its growth is, and the more clot it contains, obviously the better adapted must it be for the compression treatment. But the histories of the cases hitherto known will prove that this treatment is not limited in its application only to these simpler forms of the disease. This proposition is proved by the cases recorded of successful pressure under the care of Sheppard, of Worcester, and Kerr, of Canton, as well as those of Rouge and Humphry, referred to in the last lecture.

Another case, which excited much interest here at the time of its occurrence, is the following, for the notes of which I am indebted to Mr. Gay, who was originally consulted in it, and to Mr. Coles, who superintended the application of the pressure. The patient was a city merchant, who had been in a railway carriage which ran up an embankment. He and two other occupants of the carriage scrambled very hastily out of the window, head foremost. He did not perceive that he had sustained any injury beyond a shaking. About a fortnight afterwards, while shaving, he perceived a pulsating tumour at the root of the neck on the left side. It was about as large as a walnut, and was accompanied with aneurismal pulsation and an unusual bruit described as being of a whistling kind. It was situated on the carotid artery, a little above the bifucation of the innominate; but there was room to push the thumb or the pad of an instrument down behind the sterno-clavicular joint and stop the pulsation, nor did this give him any peculiar inconvenience. The diagnosis was made by Mr. Coulson and Mr. Gay, who first saw the case, and was subsequently confirmed by Mr. Buxton Shillitoe, Sir J. Paget, Sir W. Fergusson, and Mr. Prescott Hewett, who were called in consultation. There can therefore be no reasonable doubt of the aneurismal nature of the tumour. Pressure was decided on, and was at first applied by means of a frame, to which was attached an

arm, movable in every direction by rack and pinion joints, and carrying a small pad. This was tried for four days and nights. and the proper application of the pad stopped the pulsation; but it was thought that the instrument was liable to become displaced by the movements of the patient's body, and accordingly, at a consultation between Mr. Gay and Sir J. Paget, it was decided to resort to digital pressure. In this the services of my friend Mr. Coles were employed, and the treatment was commenced on June 4th, 1867, rather more than a month after the first discovery of the aneurism. Relays of students were obtained to keep up pressure night and day; the patient was placed on an Earle's bedstead, and digital pressure was commenced about 6 p.m. He bore it with much fortitude, and it was kept up pretty continuously till the evening of June 7th—*i.e.*, for four days and three nights—the patient sleeping at intervals. The excoriation of the skin and its extreme tenderness now compelled the discontinuance of the pressure. It was resumed at 10 a.m. on June 10th, but after six hours the tenderness of the skin again compelled its discontinuance. In the interval the acetate of lead had been given in two-grain doses three times a day. The effect of the pressure had been to abolish the bruit in the tumour; but this returned on June 14th. The skin, however, being still too tender to bear pressure, the acetate of lead was continued until June 24th, when digital pressure was resumed. In the interval a consultation had been held, at which the tumour had been somewhat roughly manipulated, but without any methodical attempt to effect the dislodgement of any clot, and with no symptoms of embolism following. On the day above named (June 24th) digital pressure was resumed, as before, at 10 a.m.. and was kept up till June 27th—*i.e.*, a little over three days, when it was discontinued finally. It was not, however, until a month from this time,—viz., July 27th,—that the pulsation in the sac disappeared, and the reduction in the size of the tumour enabled him to button his shirt collar. From this time the tumour gradually diminished, and in about two months had quite disappeared. He was examined by Mr. Gay last March, and was found in perfect health, with no trace of aneurism whatever, and the carotid conveyed blood and appeared to be of the same calibre as the artery of the opposite side.

Two views may, I think, be taken of this case,—namely, (1.) that it was a small sacculated aneurism, and that the application of the pressure produced a certain amount of deposit of soft clot in the sac, which afterwards went on to complete obliteration under the influence of rest and appropriate regimen, aided, very possibly, by the administration of the acetate of lead; or (2) that the artery was merely dilated, as

we so often see in the right carotid near its root, and that the pressure, by temporarily relieving the dilated coats of the artery from the force of the circulation, enabled them to contract again. It would be rash, and even absurd, to dogmatise about a case which one did not see; and the latter view appears probable to Mr. Gay, who was in attendance on it. But I may perhaps be allowed to observe that such aneurismal dilatations are not usually accompanied by anything like a distinct bruit, nor are they known to be susceptible of cure by any method; while the gradual disappearance of pulsation after a vigorous application of total compression, and the subsidence of the tumour in a longer or shorter space of time, is exactly what has been noticed in other cases of cure of aneurism by rapid pressure, as in Dr. Murray's and Mr. Durham's cases of abdominal aneurism. In the latter it was more than a month after the discontinuance of pressure before the pulsation finally ceased. In those cases there could be little doubt that the effect of pressure had been to allow the sac to contract on its contents, and to produce a deposit of soft clot in the sac, and that this gradually hardened and increased until it filled the sac, or that by some lucky accidental displacement of a portion of it, the mouth of the sac became obstructed, and the cure was thus produced. Whatever view we take of the case just related, it must possess very great value as showing the possibility of curing by milder means the very class of cases for which Wardrop performed and recommended Brasdor's operation. It would have been very interesting in this gentleman's case to have noted the effect of distal pressure, but, as far as I have been able to ascertain, this was not done.

I have met with no case on record, nor heard of any in private, where the rapid method of pressure has been used in carotid aneurism; yet I can easily imagine circumstances in which it might be advisable; where the patient cannot tolerate the pressure without chloroform, yet the aneurism is of no large size, and appears to contain some clot. The rapid cure of many cases of small aneurisms under pressure when clot has previously been deposited appears to prove that, in our Hunterian preparation (1685), a cure might have been attained in a very few hours.—*Lancet, June* 21, 1873, *p.* 867.

55.—ON ANEURISM OF THE VERTEBRAL ARTERY.

By Timothy Holmes, Esq., M.A., Professor of Surgery and Pathology to the Royal College of Surgeons.

The occurrence of spontaneous aneurism of the vertebral artery in the neck has not as yet been noticed, so far as I am

aware; but traumatic aneurisms of this artery are not so very infrequent, and we possess already records of eleven cases in which the carotid artery has been tied—in four for wound and in seven for traumatic aneurism of the vertebral.

It is therefore worth while, in discussing the practical surgery of cervical aneurism, to see what were the leading features of these cases, and how such an error could in future be avoided. For there can be no question that the ligature of the carotid in a case of vertebral aneurism must be fraught with double mischief. In the first place, by throwing the strain of the anastomising circulation on the vertebral, it tends to aggravate the disease it was meant to cure; and in the second, if the circulation in the wounded vertebral artery is interrupted before the operation (which to some extent it almost certainly must be), the stoppage of the supply from the carotid artery is rendered doubly perilous to the nutrition of the brain.

Of these cases, the one which is most fully reported, and which best illustrates the diagnosis and treatment of vertebral aneurism, is that recorded by Lücke in the eighth volume of Langenbeck's Archiv, in which a traumatic aneurism formed at the nape of the neck after a stab. As compression on the carotid checked the aneurismal pulsation, the common carotid was tied. This produced no effect. Repeated injection of perchloride of iron into the tumour produced suppuration and the rupture of the sac. The latter was then laid open, and the open mouth of the vertebral artery was plugged near the foramen magnum. All went on well as far as the aneurism was concerned. There was no more bleeding, but the patient died from hemiplegia and softening of the brain, the result apparently of the operation on the carotid.

In commenting on this interesting case, Lücke remarks on the error into which he was led by the fact that compression on the carotid checked the aneurismal pulsation. He explains this by supposing that the compression of the carotid artery acted simultaneously on the vertebral, and points out that this may easily happen if the vertebral (as is occasionally the case) enters a foramen in a higher vertebra than usual. It is a little singular that he seems to have omitted to ascertain whether this was so in his patient when he had the opportunity at the post-mortem examination. But other writers have noticed the possibility of checking the pulsation of vertebral aneurism by pressure applied in the same way as for aneurism of the carotid, even in the normal relations of the vertebral.

On the subject of digital compression of the vertebral artery Chassaignac speaks as follows:—"As the vertebral near its origin lies immediately behind the common carotid, between it and the spinal column, the simultaneous closure of both arteries

is inevitable [*i.e.*, when pressure is made on the carotid low down], and it is an error to think that very powerful compression is required for this purpose. In order to convince oneself of this it is only necessary to repeat the following experiment on the dead subject. After having removed the skull-cap and the contents of the cranium, taking care to cut the vertebral arteries below their junction in the basilar, tie the axillary arteries and the upper part of the abdominal aorta, then inject water through the arch of the aorta. The liquid will be shot out through the two internal carotids and the two vertebrals at the base of the skull. But as soon as even light pressure with the fingers is put on the course of the common carotid in the space extending between two and three inches above the clavicle, between the trachea and the internal border of the sterno-mastoid, the jet will no longer flow from either the internal carotid or vertebral on the side compressed, but will recommence as soon as the compression is removed. If both sides are compressed at once all the arteries cease to spirt. If pressure is made with the same force on the common carotid above the 'carotid tubercle' (*i.e.*, the anterior tubercle of the transverse process of the sixth cervicle vertebra), the space below that tubercle being left free, the jet from the vertebral artery will continue, while that from the internal carotid wholly ceases." He proceeds to show that the tubercle in in question is much higher than is generally supposed, being always from two to three inches above the upper border of the clavicle. The bearing of this fact on the diagnosis between carotid and vertebral aneurism is obvious. In a case like Lücke's or Kluyskeus', the surgeon, knowing now the true position of the tubercle on the transverse process of the sixth cervical vertebra, and knowing that pressure applied along the course of the carotid anywhere below this—*i.e.*, for two inches at least above the clavicle—will most probably stop the pulsation in the vertebral also, will not conclude that the aneurism affects one of the carotids, or some branch of the external carotid, until he has seen that pressure also stops the pulsation when applied at a higher level, or when applied to the carotid in such a way as to isolate it from other structures, as M. Rouge did. A traumatic aneurism whose pulsations are unaffected by compression so applied must be regarded and treated as vertebral.

There are several cases which are classed by surgical authors as "vertebral aneurisms," in which the fact has not been absolutely ascertained by post-mortem examination, but where the progress of the case left no doubt as to its nature. This was so in Kluyskeus' case, reported by Chassaignac from the Annals of the Medical Society of Ghent. A man had been stabbed an inch

below the mastoid process. The wound healed, and a traumatic aneurism formed, which grew rapidly to the size of the fist. All symptoms ceased at once on compressing the carotid, but at what level was not noted. The result of this compression, however, left no doubt on the minds of the surgeons that the aneurism affected some artery of the carotid system, and, accordingly, the trunk of the common carotid was exposed and tied. The operator did not notice whether compression on the denuded artery would stop the pulsation; at least, there is no note to that effect. But he noticed with great surprise, after tying the ligature, that it produced no effect on the tumour. The man recovered from the operation, but the pulsation of the tumour continued as before, except that it diminished in force during a severe attack of dysentery which occurred to him in the hospital, and much reduced his strength. Finally, after he had left the hospital, and his general health had improved, the aneurism burst, and he died of the hemorrhage some four months after the wound.

In this case there was no post-mortem examination, but the symptoms are identical with some in which the vertebral has been found to be the artery injured, such as the following, which is also quoted by Chassaignac (Chiari's case).

A man had been stabbed beneath the left parotid; the wound healed, but a traumatic aneurism formed, and grew to the size of an egg in about two months. It was taken for an aneurism of one of the posterior branches of the external carotid, and the common trunk was tied. The pulsation continued. The patient died from the operation, the cause of death being apparently pyæmia. The autopsy proved that the tumour was situated on the vertebral artery, at the precise point where it makes its curve between the first two vertebræ.

Cattolica, of Naples, has related in an Italian journal (translated in the Gazette Médicale, 1836, page 425) two cases of aneurism of the vertebral artery—one treated by ligature of the carotid, and proving fatal by rupture of the aneurism. The precise level at which the artery was wounded is not stated, but it was high up in the neck. Compression of the carotid had not been tried. In the second case the wound was immediately below the angle of the jaw. The ligature of the carotid was commenced, but when the artery was exposed it was found that pinching it between the fingers did not stop the pulsation of the tumour. The operation was abandoned. Compression and styptics were applied to the tumour, but the skin covering it was sloughing, and it would soon have burst, when the patient died of gastric fever.

In commenting on these cases the French writer points out the propriety of commencing the treatment by direct compres-

sion, which having failed, he recommends the plan ultimately adopted by Lücke—viz., to lay the tumour open and command the bleeding mouth by compression with charpie steeped in some astringent.

After the relation of his case Lücke also comments at considerable length on the treatment which should be pursued in similar circumstances, allowing that the diagnosis of vertebral aneurism has been established. The ligature of the vertebral artery at the root of the neck is a difficult and formidable operation, nor is there any probability that it would check the circulation through the upper portion of the vessel so near its inosculation with its fellow. He argues therefore, I think, conclusively against any such attempt, and still more is the attempt to tie the subclavian above the origin of the vertebræ to be reprobated. Yet I cannot say that the course which he followed—viz., the repeated injection of perchloride of iron—seems at all to be recommended. In order that this mode of cure should have any fair chance of success, it appears to me necessary that the surgeon should be able to command the affected artery both above and below the tumour. And if the injection does not succeed, or at any rate produce some marked effect, at the first or second sitting, I doubt whether it should be persevered in. To practise repeated injections into a tumour like this will almost certainly cause suppuration and rupture of the sac, as it did in this instance.

The only case of recovery after vertebral aneurism which I have found is reported by Möbus in the fourteenth volume of Graefe and Walther's Journal. It bears much resemblance to Lücke's case, being the result of a stab just below the ear. The tumour was expected every day for some time to burst. The surgeon, believing that some branch of the external carotid was wounded, exposed that artery in order to tie it, but found that pressure on the denuded artery had no effect on the pulsation, so abandoned the operation, and urged the laying open of the tumour. The patient would not consent, and ultimately the tumour ceased to beat and disappeared. No treatment was adopted except the application of cold. The cessation of pulsation was preceded by very severe pain.

Bearing this case in mind, I think we should hesitate before proceeding to any extreme measures in a case of traumatic aneurism of the vertebral artery. I do not see what operation can be practised on such a tumour with any hope of success, except that modification of the method of Antyllus proposed by Mr. Syme—viz., to make a small opening in the sac, through which the finger is introduced, plugging the hole and preventing hemorrhage, while the surgeon feels for the point at which the blood-stream enters the sac, and where the pressure

of his finger checks the pulsation. Having this under command, the sac may be freely laid open, so as to expose the mouth of the vessel, and a ligature may possibly be cast upon it. Even if the vessel is divided too near the bone to be tied, it may be possible to plug it, as Lücke did. The account of his case and of its post-mortem examination, points very strongly to the conclusion that his patient might have recovered but for the ligature of the carotid, even in spite of the damage done by the injection of the perchloride of iron; while abstinence from all active treatment might very possibly have led to the same fortunate result as in Möbus's patient.

The effect of compression applied over the lower part of the carotid ought to be carefully noted in any case of supposed vertebral aneurism; and if this stops or greatly reduces the pulsation of the tumour, I see no reason why it should not produce a cure if the patient can tolerate it.

The conclusions, then, to which present experience points on the subject of vertebral aneurism may be thus summed up:—

1. A traumatic aneurism may be taken to be vertebral when it is situated in the course of that vessel, and when its pulsations are not commanded by compression of the lower part of the common carotid.

2. When a traumatic aneurism is situated as above, and its pulsations are commanded, however completely, by pressure on the common carotid low in the neck, it ought not to be treated as being carotid, or as affecting a branch of the carotid, unless it is clearly proved that its pulsations are stopped by pressure applied above the level at which the vertebral ceases to be compressible—*i.e.*, above Chassaignac's "carotid tubercle."

3. An aneurism diagnosed as vertebral may be treated by compression (gradual or rapid, as the case demands) of the root of the vertebral artery in the neck, if this is found feasible.

4. If indirect compression will not stop the pulsation, or if it cannot be borne, the tumour should be subjected to direct compression and refrigeration, to which internal remedies may be added; and possibly the subcutaneous injection of ergotine may be of use.

5. If these means fail, and the tumour appears likely to burst, or if it has burst, the sac should be opened with all due precaution, and an attempt made to tie or plug the wounded artery.

6. A wound known or suspected to be of the vertebral artery should be treated either by direct pressure or by ligature of the vessel in the wound.—*Lancet, July* 26, 1873, *p.* 105.

56.—SUGGESTIONS FOR A MORE SCIENTIFIC METHOD OF TREATING REDUCIBLE HERNIA.

By CARSTEN HOLTHOUSE, Esq., Surgeon to the Westminster Hospital.

To keep the pad in position, either a band or a steel spring is necessary; of the former I shall have to say something at a future time, at present I shall confine my attention to the latter.

For convenience of description, I shall call that part of the spring which is continuous with the pad or head of the truss, the neck; that which immediately succeeds the neck and grasps the haunch, the shoulder; next will follow the back of the truss; and lastly, the tail. The neck will include all that part of the spring between the pad and the anterior superior spine of the ilium; the shoulder, that between this process of bone and the sacro-iliac joint of the same side; the back will correspond with the spine, and extend from one sacro-iliac joint to the other; and the tail, from the latter to the end of the spring. In the double truss there will, of course, be two necks and two shoulders, a back, and no tail.

There are four varieties of springs made use of by truss-makers: the German, French, English, and what I will venture to call the forceps spring; each differing in their curve or set, and somewhat in their mechanical action.

The German is characterised by the following peculiarities:—It is very strong, but slightly elastic, and its curve is as nearly as may be that of the outline of the part of the body which it encircles; thus it belongs to the class of what are called open springs, maintaining the same form when off as when on the body, and requires scarcely any unfolding when applied. By dint of great force it can be partly uncurved or made more open, or incurved or less open; and in neither case is its elasticity sufficient to restore it to quite the original curve. Its action, as exemplified by Egg's, Miller's, and Coles's trusses, is to support and resist, but to press only slightly, though, of course, it can be made to do so very powerfully by increasing its curve. Egg describes his truss in the following words:—"Supposing that the body from A to B is 5½ in., the truss is 5¼ in.; thus the pressure is but slight, and, when the truss is properly adjusted, scarcely perceptible." From the great strength of this spring, its resisting power must be considerable; but as its curve or set does not admit of a nearer approximation of the pad to the back of the spring than that just mentioned (the assumed antero-posterior diameter of the body being

5½ in.), it follows that, if the abdomen should recede to, say, 5 in., it would not only cease to exert any pressure, but would actually not be in contact with the abdominal wall. The difference, therefore, between ¼ in. and ½ in. is too little for security; and, to render this kind of truss efficient, the spring must be incurved to a greater extent than ¼ in., when, from its very slight elasticity, the pressure would be too great for comfort. Hence the German spring, *per se*, can rarely be used advantageously; combined, however, with other arrangements as carried out by Miller and Coles, it is, perhaps, the most useful of any. In the trusses of these makers, the spring is of the half circle type, and acts more as the carrier of the pad than its compressor. In Miller's this is especially the case, the spring being cylindrical and exceedingly strong; and from its being applied to the side of the body opposite to the hernia, it has great firmness and steadiness, and no tendency to slip off the haunch. Coles's is applied to the same side as that of the hernia, and is less strong and resisting, and would not keep in place without a connecting strap. This strap subserves the same purpose as that of the ordinary truss—viz., first to steady the instrument; and next, if buckled tightly, to increase the pressure of the pad by incurving the spring.

The French belong to the class of what are called close springs, being the most curved of all truss springs; it is thin, light, weak, and very elastic; it can be uncurved into a straight line, and will immediately afterwards resume its former shape; consequently, though it is always pressing, it is but slightly resisting; certainly it can be made stronger, but in that case the pressure becomes excessive. The difference as regards the action of a French and German spring may be summed up in a few words: in the former the pressure and the resistance are equal—in the latter the resistance exceeds the pressure; the first is always pressing inwards and resisting outwards in an equal degree—the second presses very slightly inwards, but resists strongly outwards. The advantages of the French spring are, that it is more clinging than other springs, and causes the pad to bite better, so as to do away with the necessity for a thigh-strap. More power also is obtained, in proportion to the weight of metal, than with other springs; because the more a spring is uncoiled the greater force it must exert. If, for example, when its ends are separated to five inches, it should exert a pressure equal to two pounds, it would, if uncoiled to seven inches, exert a pressure of three pounds. Now, the French being much more coiled than any other spring, it follows that to bring its pad to seven inches from its back it will have to be uncoiled to, say, nine inches; it will therefore exert a greater pressure than a stronger English or German spring

having a set of five inches and six inches and a half respectively, for to bring the pad of these to seven inches from their back they will have to be uncurved only two inches and half an inch. Thus, weight for weight and distance for distance, the French exerts the strongest pressure; owing, however, to its great curve, part of the weight may be dispensed with, and so greater lightness obtained with equal strength. This spring is less adapted for bad ruptures than either the English or German, but for slight cases it is sometimes preferred by patients to the other springs, and for a night truss it is the best of any.

The English spring is intermediate in curve between the two former, and is the one most generally used in this country. In the trusses of the City of London Truss Society the spring resembles the German more than the French, but I find its curve varies considerably in trusses made by different manufacturers, some approaching more nearly to the German type, and others to the French. Its action likewise partakes of the characteristics of both these springs, being more elastic than the former, and less so than the latter.

The last variety of spring that I shall have to speak of is what I have called the forceps spring, and of which we have an example in Salmon and Ody's truss. The curve of this, as will be seen from the figure, differs from all the preceding. When unapplied, its two ends approach to within about two inches of each other, so that the pads touch. It is strong, well-tempered, and very elastic, exceeding the elasticity of the English spring, and nearly equalling that of the French. It has a back and front pad, connected to its two ends by a ball and socket joint, and, like Miller's, is opposite-sided. Though not shown in the diagrams, which are intended to display only the "set" of the springs, this differs also from the others in not being what is called a shaped spring—*i.e.*, made to fit exactly the outline of that part of the body which it surrounds. In the latter there are other curves besides the set—viz., in the complete circle springs,—one downwards in the back, upwards in the haunch, and downwards again in the groin. In this, on the contrary, there are no such curves, and it is essential for the full action of the spring that it should not fit the body closely, but merely include it between its two ends, which grasp opposite points, caliper fashion.

In all shaped springs there is a certain loss of power, arising partly from the curves just alluded to, and partly from diffusion; much of the spring-force being expended over the hips, while in the forceps spring the whole is utilised. As regards the choice of this or that form of spring, much must depend on the shape of the patient and his idiosyncrasies; some find the concentrated pressure of four pounds less bearable than a

diffused pressure of five pounds, while others fret under the constriction of a shaped spring, though of less power than a forceps spring.

What one should always first aim at is efficient retention; and, secondly, that this be carried out with as little irksomeness as possible to the patient.

In former papers I have shown how very unsatisfactory was the treatment of reducible hernia, owing to the want of instruments manufactured according to definite rules and of definite strength and form; and I made certain suggestions as to the size and form of pads, and the strength and curve of springs, with the nomenclature which should be adopted to indicate these. But to render the treatment of this affection still more efficient, we ought to be able to combine the advantages of different instruments, to adapt the peculiarities or special features of one to that of another—a German, French, or English spring to a Wood's, Cole's, or any other kind of pad—in short, to make every spring fit every pad. To render this practicable, it is necessary that the spring and pad should not be, as they now are in the majority of trusses, practically in one piece, but in separate pieces, as exemplified in Salmon and Ody's truss, and the spring made fixable to the centre of the pad, instead of to its upper border. It is further necessary that there should be a uniform gauge for screws, as well as for the other parts of a truss; thus, instead of the present uncertain and imperfect contrivance, we should have an instrument of precision, and this branch of instrumentation would be brought up to the level of others.

If the proposals in these papers were carried out, it would be easy for the surgeon either to supply his patient with the instrument best adapted to his case, or to keep a small stock of pads and springs by him as trial trusses, and, having determined the combination best suited to his patient, to give the order accordingly.

There are two other points to be attended to in the ordering of a truss—viz., the amount of pressure which the pad should make, and the angle at which it should be set on the spring, this being such as to ensure its lower border being parallel with the inguinal fold, and its surface lying flat against the abdominal wall when the patient is standing upright. As regards the first point, I have already shown in a former number of the Lancet that truss springs are gauged from about No. 15, which is exceedingly strong, up to 20, which is very

weak. The strength, then, can be always indicated, by simply sending the number of the spring; this, however, does not afford us any exact information of the degree of pressure which it will exert, because this depends, as pointed out in the paper just referred to, not only on the thickness or gauge of the spring, but also on its length and set, and the extent to which it is unecurved. Assuming now that a common circular spring truss is required, it will be necessary to bear in mind that the greater the circumference of the patient's body, or, in other words, the greater the length of the spring, so much will its strength be diminished relatively, and that a 32-inch spring, of 17 gauge, is far stronger than one of 38 inches of the same gauge. So, likewise, if the antero-posterior diameter of the body measure eight inches in one individual, and nine in another, the same spring will exert a greater pressure in the latter than in the former.

As regards the pad being set at such an angle to the spring as shall ensure its lower border lying parallel with the inguinal fold and its flat surface against the abdomen when the patient is erect, there exists no mechanism for this purpose in the common truss; as it is made, so it must remain; hence the above angles ought to be carefully measured, and the truss-pad made to correspond. For the purpose of ascertaining these points, as well as for indicating the form of the pelvis, I have had constructed a half-spring skeleton truss, in which all the above points can be ascertained with great exactness and by a very simple mechanism, and as the instrument will be exhibited in the museum of the British Medical Association, I subjoin a short description of it. It consists of a half-circle wire spring of only one curve, and of a back and frout pad, which slide along the spring, and can be fixed at any point and at any axial angle by means of a small side screw; thus the length of the spring required for the truss is indicated, and the horizontal, axial, or surface angle of the pad determined. The angle which the long axis of the pad should bear to the spring is ascertained in the same way as the axial angle—viz., by fitting the instrument on the patient, and fixing it at the required angle by means of a terminal screw. Where a shaped spring is desired, instead of a simple curved one, all that is necessary is to note the points at which the wire is not in contact with the body, together with the size of the interspaces, then, by placing it on a piece of paper, the shape of the outline can be drawn, and the exact curves indicated, with the greatest precision. The instrument is manufactured by Messrs. Maw, Son, and Thompson, Aldersgate Street.—*Lancet, July* 19, *and August* 2, 1873, *pp.* 75, 145.

57.—ON THE RADICAL CURE OF HERNIA.

By JOHN WOOD, Esq., F.R.S., Professor of Surgery in King's College, and Surgeon to King's College Hospital.

I have long thought that we might, in favourable cases, safely do more than we now attempt, to prevent a return of the protrusion after the operation for the relief of strangulation. After performing operations for the radical cure more than two hundred times, I had grounds for the belief (which other operations on the peritoneum also favoured) that, in a healthy subject, the peritoneum might be dealt with as freely and as safely as any other tissue; and, also, that the chances of bad results from peritonitis would depend upon the injury sustained by the bowel in strangulation, rather than upon any way of dealing with the peritoneal sac and parietes after the strangulation had been relieved, provided that due drainage be secured. In cases where the bowel and omentum are congested only, in young and healthy lads, with strangulated inguinal hernia, I concluded that the attempt would be justified, and would probably be successful. If so, the advantage of preventing a life-long trouble and danger from rupture by the operation which relieves strangulation is obvious. The kind of cases I selected for such an attempt, and the nature and results of the proceedings, will be best conveyed to your minds by a short *résumé* of the three cases where I have had the opportunity of carrying it into practice.

On June 29th, 1868, was admitted into King's College Hospital, a young man, A. F., aged 21, of Warden Road, St. Pancras, with a strangulated right oblique scrotal hernia. The tumour had occurred suddenly from lifting. It was of the size of the fist, and had been strangulated twenty-four hours. He had constipation, violent retching and vomiting, not fæcal, quick pulse and anxious face, pain in the epigastrium, and much pain and tenderness in the tumour, upon which several ineffectual attempts at taxis had been made. I put him well under chloroform, and made a fair and full attempt at reduction by the taxis, but in vain. I then made an oblique incision over the tumour, dividing the layers in the usual way, and laying open the sac to the extent of three inches. The sac contained a moderate amount of omentum, covering a knuckle of bowel, all congested, and the bowel slightly ecchymosed, and there was about an ounce and a half of bloody serum in the sac. The point of strangulation was at the deep inguinal ring, and it was divided by an upward cut. The bowel was then drawn down slightly and carefully examined. It presented the usual indentation, but was smooth and shining. It was

then returned, and after it the omentum, the latter being carefully spread over the deep opening. The sides of the sac were then brought together, so as closely to embrace the cord over the whole length of the canal. The handled curved hernia needle, used in my operation for the radical cure, armed with silver wire, was then employed to bring together the sides of the sac, together with the aponeurotic structures along the whole length of the canal, in the way I have described in my work "On Rupture." A good view of the conjoined tendon was obtained, and the wire fixed in it in two places. The loop and ends of the doubled wire were then brought out at the upper and lower ends respectively of the incision. Four interrupted wire sutures were then placed through the skin between these points. The wound was dressed with carbolic lotion, and covered with gutta-percha skin and cotton wool, powdered with McDougall's powder to absorb the discharges. The interrupted sutures were removed on the fourth day, primary union having been by that time obtained throughout, except where the thick wires passed through the extremities of the wound, and effectively kept up the drainage. These were kept in for ten days. There was not the slightest sign throughout the case of the peritoneum being inflamed, and the abdominal tenderness which was present at the time of the operation passed away entirely. The sickness ceased directly after the operation, and the bowels were opened naturally two days afterwards. Erysipelas being present at this time in the ward, the patient was attacked by it on the sixth day. A partial re-opening of the wound was the consequence, together with suppuration in the fundus of the sac of the hernia. The pus passed freely along the wires, and there was no burrowing.

The suppuration in the sac caused obliteration and shrinking of that structure, and the testis was drawn by the subsequent contraction into the upper part of the scrotum. Some delay in the convalescence was then produced, but finally the patient was discharged, wearing a light truss, August 15th, 1868.

During the first year after the operation I saw him twice or three times. There was no cough-impulse whatever when last seen, all the parts being very firmly braced up in the groin and around the cord. As he was repeatedly enjoined to present himself at once if any pain or weakness showed itself, and seemed fully impressed with the danger from strangulation which he had escaped, I have, I think, some right to conclude that there has been no return. The difficulty of following cases for a number of years, in the nomadic habits of the population which furnishes the most numerously favourable cases for the radical cure, is one which I experienced in this case.

The next case was in a patient who had wished much to be operated on for the radical cure before the rupture became strangulated, in consequence of the impossibility of keeping it up with a truss, and the consequent disability from following his employment. I had refused to do so because of his age and the direct nature and great size of the openings.

M. W., aged 52, had suffered from a large, right, direct inguinal rupture for twenty-six years, which, though at first entirely reducible by rest, and the recumbent posture, could not for many years past, on account of the large size of the opening, be entirely pushed back or kept up by any of the great number of trusses which he had tried. He was admitted April 14th, 1872. Had been vomiting for eight hours, and was very weak and low. The tumour filled the scrotum, was of the size of the two fists, and was tense, tender, and painful, with the skin of the scrotum red and inflamed from handling. A somewhat prolonged trial of the taxis being made under chloroform, and after the application of ice for three hours, and by inversion of the patient without success, the operation of herniotomy was proceeded with by a single oblique incision four inches long. On opening the sac, a very large mass of omentum was found adherent at the neck, but not elsewhere, and covered by it was a large fold of small intestine, dark and congested, but still smooth and glistening. After division of the strangulating band at the deep opening, the bowel, being found not much damaged at the strictured part, was easily reduced. The omentum was congested, but not gangrenous, and, on account of its size and the adhesions, somewhat recent, at the neck, could not be fairly reduced or retained in the abdomen. The mass of fat, about three-quarters of a pound in weight, was then cut off close to the adhesions at the neck of the sac. Such vessels as bled were tied separately, and the stump being enclosed between the sides of the serous sac, four interrupted sutures of carbolised hemp were applied through the sac and tendinous parietes, and tied up so as to embrace the cord pretty closely. The two uppermost sutures were passed through the stump of omentum itself. These, and the ligatures upon the arteries, were then tied up in one knot, and left hanging out of the lower angle of the wound to act as a drain for the discharges from the face of the omental stump. Lateral pads of lint and a spica bandage were applied, and the patient placed with his knees drawn up and shoulders raised, and five grains of pilula saponis cum opio given every four hours. The vomiting and distress at once ceased; there was no tympanites and but little tenderness, and the bowels acted freely on the third day. An abscess subsequently formed in the lower part of the enormous sac, and a small slough

appeared in the scrotum over the testis. Through the aperture left by this, the gland showed some tendency to protrude, but was easily kept in place by strapping and pads. By the use of a drainage-tube through the lower part of the primary incision, and out at the aperture left by the slough, the matter flowed away freely, and there was no tendency to burrow. The ligatures and sutures had all come away by the 29th of April, and on the 22nd of June he left the hospital, with the parts much thickened, firmly braced up, and wearing a truss with a large ring pad. I have seen this patient several times since then—once in this present year. There was then a bulge felt in the groin when he coughed (and he has always a bad winter cough); but there was no sign of any descent in the scrotum, and a light truss kept him very comfortable and able to do a good day's work without inconvenience. He also promised to come to me at once if he had any more trouble with it.

The third case was that of E. B., a waiter, of Surrey Street, Strand, aged 21, in whom the rupture, a right oblique scrotal one, had not been observed till the day before his admission, when, after lifting a heavy box, he felt pain and sickness, and felt a lump in his scrotum. A doctor to whom he applied gave him an aperient, after which he began to vomit, the pain was worse, and the tumour increased in size and filled the scrotum. This rapid appearance and increase of the rupture were afterwards explained by its proving to be a congenital hernia, with the sac formed by the tunica vaginalis, of which these peculiarities are very characteristic. On the 28th March, twenty-four hours after the occurrence of the rupture, the symptoms were very intense, and the taxis, with inversion and chloroform, having been fairly tried, after two hours' application of ice-bags, I performed herniotomy exactly as in the first case I have described. The omentum and bowel were found in the tunica vaginalis, both congested and slightly ecchymosed, with some effusion of bloody serum. The knuckle of bowel was strangulated, not only by the deep ring, but also by a band of the omentum, necessitating the drawing down of both until the constricted part could be seen and released. It was a case in which the bowel might easily have been passed into the abdomen with the stricture undivided. After the operation the patient was at once relieved; the bowels were opened normally on the third day; no tympanites or tenderness became apparent. The central part of the incision healed by adhesion; a little discharge passed along the wires, which were withdrawn on the fourteenth day after the operation. He was sent out of the hospital April 26th, 1873, with a firm adherent cicatrix, and no bulge whatever. On the 12th May he showed himself, wearing a light horse-shoe pad truss; not the slightest

sign of a return was evident. This patient is here to-day awaiting your inspection.

There are also other patients upon whom the operation for the radical cure has been performed. One of them—the largest rupture I have ever succeeded in curing—was operated on eleven years ago, the patient having followed afterwards a very laborious occupation as a porter in Covent-garden market. He wore a truss for only nine months after operation. I have brought him here to-day as a proof of the possibility of curing even a very large rupture by operation, and of curing it permanently, so as to relieve the patient of the trouble and expense of wearing a truss even under the pressure of very hard daily work.

It has been said, with respect to this operation, that evidence is wanting as to the permanency of the cure, and I am free to confess that it is exceedingly difficult to watch a couple of hundred cases for the space of ten or eleven years. The constitution of human nature is such that you cannot hope for the generality of patients to show themselves occasionally for this length of time, or even to write, if they are not further troubled with the ailment of which they have been cured. It requires that powerful spring to gratitude which was said by the cynical French philosopher to consist in "a lively sense of favours to come," which is wanting in a case where there is no more for the doctor to do. I have found that the unsuccessful cases are more likely, by the law of gratitude just enunciated, to return upon your hands than the successful ones are, as a bad shilling expects to be replaced by a good one. I think, therefore, we have the better right to the position of reckoning in the same proportion of failures and cures cases which have not been seen twelve months after the operation, as those which have been examined after that time.

Now, of 188, most of them unselected, cases of inguinal hernia—of the 200 and upwards I have operated on to the present time, of which I have notes, including seven females and four cases of double rupture, both operated on,—in 107 cases the results are more or less perfectly known. I find that 51 of these were more or less unsuccessful, of which 42 returned in the first year after operation—that is, the great majority of these were such as would do without wearing a truss after the first year. By far the greater number were so much improved that they were made comfortable by a truss, which was not the case in most instances before the operation. Some, but not many in number, were as bad as before the operation. Mr. Kingdon, of the London Truss Society, kindly forwarded to me in 1863, the names of three of those who had applied to that institution since the operation for the supply of a truss. 56 out of

the 107 were cases which continued to be successful subsequently a year after the operation, as ascertained either by direct examination by myself, seen by other surgeons, or satisfactory to the patient himself, and either wearing no truss at all, or only occasionally, as a precaution, after the first year from the operation. Of these, seven were noted from thirteen to twenty-one months after the operation, seven from two years, seven from three years, seven from four to six years, seven from six to eight years, and four from nine to eleven years. Reckoning operations on both sides and repetitions of the operations, I have done the operation more than two hundred times. Out of these I have had three deaths—one from pyæmia, one from erysipelas, and one from peritonitis. These have been made public to the profession on more than one occasion, because I judged it right and fair that in an operation of this kind the facts should be made known as far as possible. In the last case, as shown by the post mortem examination, the peritonitis was found not to have originated in the parts operated on, but in a knuckle of bowel which had been lodged in the hernia before the operation, while the patient was wearing a strong truss. The cases in which any signs of peritonitis were observed were not more that about twenty in the whole number. One and a half per cent. is not a high average of deaths from purely accidental surgical incidents, and there are very few operations of the kind—as, for example, for the removal of deformity, the cure of prolapsus of viscera, or of hemorrhoids—which could show more favourably either in this respect, or in respect to the somewhat severe test of the length of time in which they have been known to be without a relapse after the operation; and since 42 out of the 51 known unsuccessful cases proved to be so within the first year after operation, and most of the cases were examined once or more at various intervals of time after the operation, I think that in respect to this point we have a right to claim the probability of more, and the certainty of at least as many, good results for the 81 of which I have not been able to get notes after the first twelve months, as for the 107 in which I have done so. Under the age of twenty-one years the results in known cases are much more satisfactory, and amount to seventy-five per cent. of successful cases. But of dry statistics you will think that I have given you, perhaps, more than enough.

The determination of the question as to whether the operation for the radical cure is an appropriate alternative to a life-long wearing of a truss, and a valuable supplement to the slow and very uncertain cure by truss pressure, will continue to depend upon the age, habits, circumstances, mode of life,

and, to some extent, the cruel experience of trusses, and wish of the patient after having the matter fairly put before him, and, perhaps, nearly as much upon the anatomical knowledge, skill, energy, and experience of the surgeon, or his disposition to the finality frame of mind I have alluded to. In any case, whether universally, or only occasionally resorted to, it forms, I think, a valuable addition to the resources of surgery.—*Lancet, Aug.* 9, 1873, *p.* 185.

58.—ON A METHOD OF OPERATING IN CERTAIN CASES OF STRANGULATED HERNIA.

By THOMAS ANNANDALE, Esq., Surgeon to the Royal Infirmary, Edinburgh.

[The method of operating to which attention is directed in this paper has been previously suggested, but has never been practised on the human subject.]

The principle of this method consists in making a small incision through the abdominal walls, and opening the abdominal cavity *near* the hernia, instead of cutting into the hernial sac itself, or exposing it. One or more fingers are then to be inserted into the abdominal cavity, and the protruded structures contained in the hernial sac drawn back from within.

The chief objections which may be reasonably urged against this proceeding are—1st, The risks likely to follow opening into the peritoneal cavity; and, 2nd, that certain conditions of the hernial contents, such as gangrene, or firm adhesions, would prevent the operation being successfully carried out.

The first of these objections is not, in my opinion, very serious, but the latter is so, and therefore I would advise the method only in *certain* cases of strangulated or obstructed hernia.

The cases which appear to me to be most suitable for this operation are:—

1. Umbilical herniæ in the adult, especially if the coverings of the hernia are thin or ulcerated, or the hernia large.

2. Large inguinal or scrotal herniæ, which have been recently reducible.

3. Herniæ in other regions in which, owing to special conditions, the usual operation cannot satisfactorily be performed.

This operation is, however, only suggested when in these forms of herniæ the condition of strangulation or obstruction gives rise to symptoms which *absolutely* necessitate operative interference, and when signs of gangrene or a history of old standing irreducibility are absent.

The incision into the abdominal cavity does not require to be more than from one to two inches in length, and it is quite possible, as I found in the case related, to examine the condition of the intestine contained in the sac, by carefully drawing it, bit by bit, through such a wound. If thought right, it is also quite practicable and safe to divide, subcutaneously through the same wound, any constricting band or agent which may be felt after the hernial sac has been emptied of its contents.

In the case reported, I was induced to perform this operation because I was aware that the usual operation is very unsatisfactory in cases of large and strangulated umbilical hernia.

The operation completely relieved the strangulation and obstruction; the bowels being freely moved shortly after. No symptoms of peritonitis followed the operation; but the constant vomiting, and the inability to take the slightest nourishment for some days before, together with a fatty condition of the heart, caused, in my opinion, the exhaustion which proved fatal.

Case.—On the 23rd of April last, Dr. David Gordon asked me to see with him Mr. B., aged 61. The patient, who was very stout, and measured round the waist fifty-one inches, had suffered from a large umbilical hernia for many years. Three days before my visit he was suddenly seized, when travelling, with a pain in the abdomen, vomiting, and all the symptoms of *complete* intestinal obstruction. For two days he was treated at home by his friends, but on the morning of the third day he became alarmed at his condition, and sent for Dr. Gordon, who, at once recognising the serious nature of the case, requested a consultation with me.

An examination determined the presence of an umbilical hernia, the size of a large orange. There was considerable inflammation of the superficial textures round the hernia, and a superficial slough of the skin covering the hernia itself. The hernia was not tense, but on laying hold of it its contents felt somewhat hard, and they were not reducible. The abdomen was swollen and tympanitic, and the patient had pain, stercoraceous vomiting, and all the other signs of intestinal obstruction well marked. Attempts to reduce the hernia were quite ineffectual, and an operation appeared to give the only chance of relief. The patient's consent to the operation having been obtained, and chloroform administered, I made an incision three inches in length in the mesial line, midway between the umbilicus and ensiform cartilage; and after cutting through three inches of superficial fat, I carefully opened into the abdominal cavity, to the extent of about one inch and a half. A knuckle of gut at once protruded at the opening, and passing

my fingers along the continuation of this portion of intestine, I found it passed into the hernial sac. The sac contained several inches (six at least) of this small intestine, which felt somewhat matted together, and also was adherent to the sac; the neck of the opening into the sac was slightly felt as a firm ring, but this ring was not very tight. Having determined these conditions with the finger, I gently drew open the intestine, which passed into the sac; and was pleased to find that the soft adhesions gave way and the whole contents of the sac were thus drawn out. As each portion of intestine contained in the sac was in this way drawn out, it was brought out through the abdominal wound and carefully examined. After the sac had been completely emptied, I introduced a probe-pointed knife through the wound, and freely divided, subcutaneously, the firm ring at the neck of the sac. The intestine contained in the sac was slightly congested in appearance, and on its surface were the remains of some recent adhesions, but there was no trace of any general peritonitis. The abdominal wound was closed by means of deep and superficial stitches, and antiseptic dressing applied.

Three hours after the operation, the patient's bowels were freely moved, and all symptoms of obstruction had disappeared. Next day he was weak, and the bowels were again slightly opened, but he complained of no pain or other bad symptom. The exhaustion, however, continued, and he died somewhat unexpectedly about forty-eight hours after the operation.—*Edinburgh Medical Journal, Sept.* 1873, *p.* 209.

59.—ON COLO-PUNCTURE.

By Dr. HECTOR C. CAMERON, Surgeon to the Royal Infirmary Dispensary, and to the Lock Hospital, Glasgow.

[The subject of flatulent distension of the bowel is one of very great importance, as it may occur in every class of case, whether medical, surgical, or obstetrical, and prove the source of great suffering to the patient and of danger to his chance of recovery. There have been cases of puncture of the bowel by a fine trocar, published from time to time, but the success attending the operation has not been very encouraging, although the temporary relief given may have been great. The advantage of puncture of the bowel in acute strangulation has been very clearly shown by Mr. Bryant, to whose paper it may be worth while to refer (*Retrospect*, vol. lxv. p. 219).]

Mr. S., about 45 years of age, had been under my care for some months previously, suffering from a surgical affection in the right foot. An operation undertaken for the cure of this

resulted in an inflammation extending up the limb, exhibiting itself by the symptoms of hardness and tenderness to the touch, for the most part in the course of the superficial veins of the leg and of the femoral vessels. This was subdued in the leg and lower part of the thigh by means of frequently renewed fomentations, and the exhibition of some mild antiphlogistic treatment. The neighbourhood of the groin, however, and upper part of the femoral region, remained swollen and painful, and the general feverish symptoms, which were very severe, continued comparatively unabated. In the course of another day or two, he began to complain of tenderness to the touch of the abdomen, and tympanitic swelling commenced. This was preceded by one or two sharp rigors, and accompanied with frequent, copious, and watery stools. The pain in the abdomen was soon severe and constant. This state of matters was somewhat insidious in onset, and obscure in origin, but was, we thought, probably an extension of the inflammation which existed in the right leg and thigh. At this juncture, Dr. M'Call Anderson saw him daily with me in consultation. The patient's condition soon became very precarious. The tongue was hard and dry; the pulse varied from 120 to 140; thirst was excessive; sickness occasional; while frequent rigors occurred, followed by most exhausting perspirations. The abdominal distension was extreme, causing pain, inability to take nourishment, and a distressing sense of weakness and breathlessness. It was always to a certain extent relieved by a motion from the bowels, but the gas soon re-accumulated. None of it could be voided, except when the patient was on the bed-pan; and, because of the comfort he obtained for a short time after an evacuation, he was led to encourage their occurrence. As in this way they were frequent and always copious, it soon became very obvious that this continued diarrhœa was fast undermining his strength. Finding, therefore, that the bowels could be almost completely controlled by the injection of a drachm of laudanum in starch, given night and morning, Dr. M'Call Anderson proposed that we should by this means stop the diarrhœa, and treat the increased accumulation of gas by the operation of colo-puncture. (All the usual remedies for the dispersion of flatulence were administered, some by the mouth, some by the rectum, others, such as turpentine, in both ways, but with no avail.)

Accordingly, on the 20th December, without rules for our guidance or warning, I made an attempt, Dr. M'Call Anderson being present, to puncture. The only canula which I could obtain, of at all suitable character, was small in diameter, but unfortunately fitted ill round the neck of the trocar, the latter having no shoulder on it at all.

Having driven them into the abdomen over the situation of the transverse colon, and withdrawn the trocar, we found that no gas escaped. I again introduced the trocar and, withdrawing the instrument a little, once more pushed it forward, but with no better success. It now became evident that the edge of the canula refused to enter the soft and yielding inflated gut, and we abandoned any further proceedings until the next day, when I obtained a more suitable instrument. This was a canula and trocar of good shape and small in diameter, but no longer than such as is commonly used in tapping a hydrocele. With this instrument I again punctured (21st Dec.), the distension being very great. The site chosen was very much the same as that of the day before, and, on withdrawing the trocar, the gas whistled freely out of the canula. After a very considerable quantity had escaped, but long before the abdomen had regained its natural shape, the escape suddenly stopped; and all attempts to obtain the re-establishment of it were fruitless. The gut in collapsing had evidently freed itself from the very short part of the canula it contained in its cavity. The patient, however, expressed himself as greatly relieved. On the following day (22nd Dec.), the distension was as great as ever; and an instrument of a satisfactory character, which had been ordered, having been obtained (the canula being $3\frac{1}{2}$ inches in length, and $\frac{1}{8}$th of an inch in diameter), I again punctured with it in nearly the same position. The gas escaped freely, and the abdomen fell fast. Anxious to expel all the gas, we kept up rather firm pressure on the abdomen; but to our disgust, before all the gas had been thus got rid of, we pressed a quantity of liquid fæces through the canula. I wiped away carefully what had passed, and, giving time for any that remained in the canula to gravitate back, I plucked it out with my thumb firmly over the orifice, so as to retain, if possible, any fluid that might still be in the canula, in the same way as one lifts fluid in a pipette.

The patient was greatly relieved in breathing, and the pulse fell considerably even before we left the room. No evil consequence ensued, and he expressed himself that evening as being very comfortable.

On the following day (Dec. 23) the operation was repeated with equal benefit, but no pressure was used, so much of the gas only being got rid of as escaped spontaneously.

Again, on the next day (Dec. 24), I punctured in the situation of the descending colon, the patient lying on his back, and the instrument being introduced parallel to the surface of the bed. On withdrawing the trocar, a small quantity of liquid fæces was forcibly shot out on to the carpet of the room

by the first burst of gas: but no more followed, and a great quantity of wind was got rid of.

On the following day (Dec. 25) I punctured in nearly the same position, but drove in the instrument at right angles to the surface of the bed, the patient still lying on his back. No fæces escaped, the evacuation of gas was most satisfactory, and the relief to the patient, as on all the other occasions, was very great.

On Dec. 26 it was not thought necessary to puncture, the flatus being relieved during the passing of a liquid stool, and not re-accumulating very rapidly.

On the morning of Dec. 27, however, the distension was again very great. About 11 a.m. I punctured in the situation of the transverse colon with good results. Shortly before mid-night of the same day my patient sent, begging I would repeat the operation. The distension was again extreme, and his sufferings from the embarrassment of the breathing very great.

I did repeat the operation, giving vent to an enormous quantity of wind, and leaving him much relieved, and inclined to sleep. On the following day we found that he had passed a good night, and, although considerable distension existed, the gas had not, we thought, re-accumulated to such an extent as to render operation necessary. Indeed, we never again required to puncture.

The distension, although it continued more or less for a week or ten days, never again attained an extreme limit; and, in a day or two after our last operation, he began to void small quantities of wind in a natural manner.

In a month afterwards, as regarded the affection of the abdomen, he was quite well, and has continued so since, although now in a most precarious state of health from serious ailments of chest and limb. Puncturation was performed, it will thus be seen, eight times in all, and on every occasion, except the first (when the bowel was not entered), to the marked relief of the patient. The operation itself, if effected with a trocar in good order, and by a quickly-delivered firm stab, is almost painless, and may be rendered absolutely so, as was done in the case under review on the last four or five occasions of puncturing by the application of a little ice and salt to the spot selected, or by the use of Richardson's spray apparatus. As to the selection of the point for puncture, I may say that I chose on each occasion a point over the position of some part of the colon, where the percussion was most distinctly tympanitic.

Only one of the little punctures gave any uneasiness whatever. Two days after it had been made, the patient complained

of its being tender to the touch, and a little redness began to show itself in an area round it of the size of a sixpence. This culminated in a minute abscess in the skin, which burst, yielded a little pus, and closed without giving further trouble.

The class of cases to which this operation is applicable naturally falls into two important divisions: first, those in which, like my own, there exists no mechanical obstruction to the escape of the contents of the gut; but where the extreme accumulation of flatus is caused and maintained by the fact that the rapidity of its generation is out of all proportion to the power of getting rid of it; and, secondly, those in which some such mechanical impediment does exist. It is in this latter description of case that the operation has, of course, been most frequently performed, because the former is of much rarer occurrence, and can generally, I am aware, be managed by other and better-known methods of treatment. Thus, a long tube passed up the gut as high as can be conveniently accomplished, when other means have failed, often accomplishes the object in view. Puncture, however, appears to me to possess certain advantages over the tube, and not, if the necessary precautions be adopted, to be accompanied by greater risks; for it appears there is some danger in the use of the tube, and at all events it will be allowed that there is considerable discomfort to the patient. The difficulty of its introduction is not always slight, and its success by no means uniform. In the cases where mechanical obstruction is the cause of distension, it must, I imagine, very generally fail, although it must be confessed that in such cases it may prove a very important aid in the diagnosis, and for this reason, if for no other, ought to be used. The great objection to the employment of the tube, however, as compared with that of the trocar, appears to me to be the undesirability of repeating the operation on more than one occasion. In the case we are now discussing I feel sure that it would not have been tolerated, even had other circumstances not rendered its use highly undesirable.

The gut has been punctured, I believe, in the case of strangulated hernia, as well as in such cases as I have referred to; punctured, of course, in that part of it included in the strangulated tumour. This has been done, but I fear with indifferent success, with the object of getting rid of the flatus contained in the strangulated bowel, and thus, by reducing its bulk, allowing of its easy return into the cavity of the abdomen. The example is one that the most of us, I fancy, would be very chary in following. Besides being open to all the objections brought against operations that provide for the return of strangulated hernia without opening the sac (that is, without

being aware what the condition of the gut is, that we are returning), it appears to me to be open to this additional objection that, if it do not succeed and operation become necessary, the free handling of recently-punctured gut might possibly lead to unpleasant consequences.

It may be thought by some that it must be a rash proceeding in a case such as mine, where the peritoneum is in a state of more or less acute inflammation, to irritate it still further by driving through it and again withdrawing so substantial an instrument as the necessary canula and trocar. As a matter of fact, however, the operation appears in no way to increase the inflammatory action; and this will cause the less surprise when we remember, that Mr. Wathen has pointed out, that in one well-known case the operation of ovariotomy was undertaken, and followed with the best results, during an acute attack of peritonitis.

It is a matter for curious speculation why no escape of fluid should take place from these punctures into the cavity of the peritoneum. The gentleman I have just referred to, in a letter in the British Medical Journal for Nov. 25, 1871, wrote in regard to this:—"The size and character of the puncture when made with a fine trocar and canula are such, I think, as to bring the chances of extravasation very low. The puncture merely separates the muscular fibres, and on the withdrawal of the canula, the muscular planes immediately re-adjust themselves, or a protrusion of mucous membrane takes place, which entirely prevents the escape of the intestinal contents, as was demonstrated by Travers in cases of punctured wounds of intestine, when the incision does not extend beyond three lines in length. The influence of pressure of the abdominal walls and contents in cases of non-protruding intestine, is clearly shown by Mr. Erichsen, when he states that fæces escape from a smaller aperture in a protruding than in a non-protruding intestine. He also refers to two cases of wound of intestine without any extravasation. A similar case occurred in my father's practice, when a man, killing a pig, stabbed himself accidentally in the abdomen; the intestine did not protrude, but the patient passed a large quantity of blood per rectum. He recovered without a single symptom of extravasation. Clinical experience, therefore, shows that non-protruding punctured wounds of the intestine of less than three lines in diameter may be said not to allow of fæcal extravasation. The lesion in colo-puncture comes so much within these limits that I venture to think it may not deter us from resorting to this method of, at the least, relief."

In addition to the opinions here given by Mr. Wathen, I think we may be re-assured in regard to any risk of fæc

escape, by reflecting upon what the condition of the gut itself must be after the puncture has been made. Having just been enormously distended, and now being thoroughly collapsed, the mucous membrane must be thrown into very redundant folds, and this of itself will act in the way of occluding the very small wound made through the wall of the gut by the operation.

An idea is very prevalent that the operation of puncturing the intestines is frequently performed in the case of cattle, but this has been pointed out to be a mistake. So general, however, was the belief, that even so well-informed a writer as Sir Thomas Watson, has repeated it in the last edition of his lectures on the Practice of Physic. He writes:—"There is one further expedient which I should recommend in those trying cases which we know (no matter how) are of necessity fatal. In cattle that are *blown* by over-feeding on wet clover, a rough procedure, that of piercing the distended bowel with a hay-fork, has often been practised by farmers with complete success. The distress from extreme distension of the intestines by wind is so intense, the craving for relief from that distress so importunate, and the comfort on obtaining it so great, that were I the subject of such pressing and prolonged torment, I should beg to have the inflated bowel eased by puncture with a fine trocar, even if I might (what is improbable) lose a day or two of painful life." Now, the truth is, that the affection of cattle here referred to (which is known as "hove" or "blown") is an acute indigestion, and the distension occurs not in the intestines at all, but in the rumen or paunch. If a herd of cattle have been, by driving, subjected to a long fast and then turned into pasture, they are very apt to give full swing to their appetites and eat hurriedly and greedily, take no time to stop for rumination, and soon the rumen becomes so distended that it is unable, as sometimes happens in the human subject in the case of the over-distended bladder, to expel its contents. Fermentation takes place, gas is evolved, and the distension rapidly increases until the animal is destroyed by the pressure on the diaphragm causing suffocation.

Tympanitis, or wind colic, as it is called, being really a collection of flatus in the bowels, does also occur, and it has been operated on successfully with a canula and trocar, but the operation appears to be attended with great danger, and is hardly thought justifiable except when the recovery of the animal is absolutely hopeless. So little apt, however, is the rumen or first stomach to suffer from inflammation or to resent operative interference, that it is customary to incise it to the extent of several inches, and to remove not only the gas, but the clover and grass which constitute the head and front of the

offending. The difference, then, between colo-puncture and such an operation as puncturing the rumen in the ox will be seen at once, and the one in no respect affords a parallel to the other.

Allow me now to sum up these remarks by indicating what I think are the precautions necessary to be taken in the performance of the operation, precautions which are indicated by the several mishaps which, as I have told you, occurred during the treatment of my case.

1. Care must be taken to secure a canula which fits very accurately indeed round the collar of the trocar, so accurately that if the finger nail be run from the point downwards it should hardly be caught at all by the edge of the canula. Unfortunately, instruments are not always so made, although any ill-fitting at this point between the two component parts of the instrument is always, and in every class of case for which it is used, not only very undesirable, but even occasionally a source of embarrassment and annoyance to the operator. I have myself felt difficulty in driving such a badly constructed instrument into the tunica vaginalis, although the parts were put upon the stretch, and the hydrocele rendered as tense as possible by being grasped in my left hand in the usual way. And if this be the case with hydrocele, it will be readily understood how much more likely it must be to occur in the case of a soft and yielding bag of air, lying unsupported, so to speak, in a cavity like the abdomen. The mishap, it is true, is one devoid of any great peril, but at all events it disappoints the operator, and pains and alarms unnecessarily the patient, who, finding no air issue from the tube, comes to think that the hopes of relief which have been raised in his mind are illusory and vain.

2. Care should be taken that the instrument is of sufficient length. The ordinary hydrocele trocar, if we consider how great a part of it is occupied in passing through the thickness of the abdominal walls, is much too short. Only a small piece of the canula remains in the gut, and, as occurred in the present case, the gut in collapsing frees itself. The canula, then, should be three or four inches long. A certain length of this may be marked off by the finger and thumb grasping just above it, and this may be driven without fear into the abdomen. The trocar may then be withdrawn, leaving a great part of the canula projecting above the skin; and as the belly falls, this may, so to speak, be "paid in" to such extent as may prove necessary.

3. Any great amount of pressure upon the abdominal walls must be avoided. When the gas is nearly exhausted, one is

tempted to use a little pressure; but it is just then that the danger of squeezing out fluid becomes greatest. If it is suspected that a quantity of fluid fæces is contained in the intestines as well as the gas, I should think it more prudent and safe to avoid pressure altogether, and to be content with such an escape as may occur spontaneously, and in all cases to make use of pressure with the greatest moderation. Although we have seen that fæces may escape through the canula and do no harm, it must be apparent to every one that such an accident introduces a grave element of danger. If I had to perform the operation in another case, I should take care to supply myself with small syringe, capable of fitting into the end of the canula; and by this means would be enabled to wash back into the bowel with a little tepid water any fluid which remained in the instrument before withdrawing it. The outside of that part of the tube which is in the gut seems to be wiped clean by its passage out of it.

4. Care ought to be taken always to puncture at that part of the intestine which is highest as the patient lies in bed. The neglect of this precaution may lead to a jet of liquid fæces being shot out with the first sudden explosion of gas, as occurred on the occasion when I tapped the descending colon; the direction of the trocar being parallel to the surface of the bed, and the patient lying on his back. The occurrence, I fancy, is somewhat analogous to the loss of fluid by the opening of a bottle of soda water or champagne held in a faulty position.

If such precautions be employed, it seems to me not too much to hope that, by means of this operation, we may occasionally save a life; or, at all events, we may do that which ought to be an object of equal importance,—when it is no longer possible to save life, we may lighten in some degree the shadow of death.—*Glasgow Medical Journal, May* 1873, *p.* 289.

60.—ON ULCERATION AND STRICTURE OF THE RECTUM.

By CHRISTOPHER HEATH, Esq., Surgeon to University College Hospital, and to the Hospital for Women, Soho Square.

One form of ulceration of the rectum is connected with fissure, and is readily cured by an incision through the middle of it. But there are cases of ulceration of the rectum of a different and more extensive character. They are most frequently met with on the dorsal aspect of the rectum, and occasionally

extend completely round the bowel within a couple of inches of the anus. Their causes are various, but I believe the most common is syphilis. This form of disease is said to occur sometimes as a result of dysentery, and it is found occasionally without any obvious cause; but by far the most common cause, as I have seen over and over again among the out-patients attending here, is syphilis. I do not mean primary syphilis. Of course, unfortunately ulceration may be found here occasionally as the result of primary syphilis, though I have never myself seen such a case. But I refer to secondary or tertiary ulceration of the mucous membrane of the rectum, which may occur, just as may ulceration of the fauces or palate, during the course of constitutional syphilis. If ulceration is also present in the latter situation, it would, of course, much clear up the diagnosis of the nature of the affection of the rectum.

The symptoms are pain of a wearying constant character and profuse purulent discharge. The amount of discharge that may come from a patient under these circumstances is really quite remarkable. I have here the paper of a patient who is attending at the present moment, and who presents a well-marked example of this affection. She is a woman aged twenty-nine, who married eight years ago, and has had one child and one abortion. She stated that she had "been passing blood and corruption from the anus, and had had much pain, but that there was nothing else the matter with her." On examining the rectum, I found, an inch and a half from the anus, a hard irregular state of the bowel, ulcerating, and pouring out a large quantity of fluid. When this woman came under my care I put down my diagnosis, syphilitic ulceration of the bowel, and proceeded to treat it on the same principles that I should treat secondary ulceration of the fauces; I mean I used mercurial applications locally and gave iodide of potassium internally. With the object of checking the discharge she has been using sulphate of copper lotion, ten grains to the pint, the direction having been given to inject a quarter of a pint and retain it for ten minutes. She has been here to-day; the discharge has wholly ceased, and the ulceration is beginning to heal, and I hope we shall be able to cure her. Of course, there is the danger, to which I shall have to advert directly, that there may be an unhealthy degree of contraction, and a stricture may result. Obviously, the first thing is to get the ulceration to heal, and by passing a bougie occasionally we may prevent the contraction. I am sorry to say we see here a large number of such cases—of married women who become infected with syphilis and get ulceration about the rectum. A large number, too, do not come until the ulceration has got into the stage of stricture.

What is to be done with ordinary ulceration of the rectum?—for no doubt there is a simple ulceration occurring in the rectum, not syphilitic. In the first place, the great thing is to put the patient in perfect rest. Keep her in bed, and give a perfectly bland diet, which cannot possibly irritate the mucous membrane. If, after a fair trial of that for some time, the ulceration does not heal, the best plan, no doubt, is to apply caustic to it, treating the ulceration as you would treat an ulceration elsewhere which would not heal. Apply strong nitric acid, or, perhaps better still, the strong nitrate of mercury, a more certain caustic. Touching the surface with a glass rod or brush which has been dipped in the solution will often effectually check the unhealthy action, and may thus induce the parts to heal.

Whatever the form of ulceration may be, it may result in stricture. You may, it is said, have a stricture without previous ulceration. There may be such cases, and I believe there is one in this specimen which I shall have to refer to, having myself operated in the case by colotomy on account of stricture resulting from deposit in the wall of the rectum. In that case the scirrhous deposit in the wall never got to the stage of ulceration.

The most common form is where you have had ulceration of the mucous membrane ending in contraction and stricture. Of course, in the syphilitic cases, which are so common, you have more than in others the tendency to contraction. The female sex is more liable than the male to stricture and syphilitic ulcerations, for in a general hospital the number of females suffering from this affection is much larger than the number of males. Whether the syphilitic poison is infiltrated through the vaginal wall or not I cannot say. Certain it is that it seems to produce effects upon the lower bowel in the female which are comparatively unknown in the male.

We have, of course, the symptoms of stricture showing themselves in difficulty in the passage of the fæces. The contracted bowel gives rise to straining, and you find that the fæces are marked, flattened into a band, or broken off into pieces, and there is often more or less of a semi-purulent discharge. On examining the bowel you readily detect the stricture within reach of the finger. You find a contracted bowel, and that the walls are more or less roughened; perhaps ulceration is still going on. By gradually and steadily introducing the finger you may be able to pass through the stricture, and feel the bowel perfectly sound beyond. When it is perfectly sound, I have often noticed that the mucous membrane is tightened up, feeling quite different from the ordinary healthy rectum;

drawn, as it were, into longitudinal bands, much more than is the healthy bowel.

The treatment of these cases must be, of course, in great part mechanical. You have to dilate the stricture, and also, if possible, to produce a certain amount of absorption of the tissue which has given rise to the stricture itself. With regard to mechanical treatment, we come to the use of bougies, which is not unattended with danger. A hard bougie should never be used; it should be softened by being dipped in hot water, and thus any damage to the bowel may be avoided. The surgeon should try first and find what size will go through, and carry out the treatment just as he would with a stricture of the urethra. You cannot be too gentle with either the one or the other.

If you have a very obstinate case of stricture of the rectum to deal with, you may have recourse, just as with stricture of the urethra, to some more speedy mode of dilatation. The two diseases are quite analogous. No one would use Holt's dilator unless the stricture were very obstinate; nor should I use this instrument—Armstrong Todd's rectal dilator—in a stricture which would dilate up to a good size. But one does meet with cases of stricture of the rectum which one cannot get on with at all, and there is no doubt that you can use this instrument, if you are careful, with very good effect. The blades of this dilator are parallel to one another, and are separated by an ingenious arrangement of cross bars worked by compressing the handles. Formerly it was found frequently to nip the mucous membrane, and often, in drawing it out, it would tear away a piece of the mucous membrane. The best plan to avoid this is to put over the dilator a finger-stall of india-rubber, which can be thoroughly greased. After the dilator is introduced, the blades can be separated so as thoroughly to stretch the stricture, and you may thus make great progress with the case. You cannot, however, be too careful with this plan of treatment. Accidents have occurred with it: one to myself. A death occurred which I attributed to the use of this instrument, although I had not an opportunity of making a post-mortem.

These cases are, I should say, excessively troublesome. You may do some good, but if the patients will not assist you by passing bougies themselves the strictures soon contract, and are really almost incurable. It has become necessary, in some cases, even to do colotomy. I have a patient now in hospital on whom I performed colotomy some weeks ago for syphilitic ulceration and stricture with great relief.—*Lancet*, *May* 24, 1873, *p.* 725.

61.—BRIEF RETROSPECT OF THE RECENT HISTORY OF LITHOTOMY, WITH AN ACCOUNT OF A NEW METHOD OF PERFORMING THE OPERATION.

By Dr. A. DAVIDSON, Physician to the Queen of Madagascar.

What is the best mode of performing lithotomy? Formerly, surgeons were pretty generally agreed in recommending the ordinary lateral operation. This unanimity no longer exists. Even those teachers whose skill and success had rendered the lateral, *par excellence*, the operation in this country, have been lately confessing their doubts whether, after all, there may not be a better way of performing lithotomy than the time-honoured operation of Cheselden.

Among the reasons alleged why this has again become one of the open questions of surgery, is the introduction of lithotrity. Since the crushing operation has come into more general use, the simpler cases of stone are less frequently treated by the knife; hence lithotomy has been reserved for the graver cases, and a corresponding increase in the rate of mortality has been the result. This applies more particularly to the practice of a few of the leading surgeons, who carefully weed out their best cases for the lithotrite, and in whose practice the cutting operation has doubtless become more hazardous. It is, after all, however, a matter of doubt, whether lithotomy is a more fatal operation now than formerly. Up to the present time, lithotrity has not been so generally resorted to as materially to affect the statistics of the operation. The late Mr. Holmes Coote remarked that, in St. Bartholomew's Hospital, the mortality has been about one in ten since the days of Cheselden down to the present time. This certainly is a remarkable fact; and all the more so if the mortality of this operation in the London hospitals generally be (as some have reckoned it) at from 20 to 25 per cent., one in five to one in four.

When we examine the annals of this operation in the past, we find that statistics then were quite as flexible as now. Raid, whose success induced Cheselden in England to adopt the lateral instead of the high operation, stated that he had operated in 1547 cases without losing a single patient. The only trustworthy statistics of his operation, preserved in the register of the College of Surgeons of Amsterdam, show that, in twenty-two cases operated on in that city, he lost four. This gives an average of one death in five and a half cases. Altogether, it is probable that the mortality at the present day is not greater than that formerly obtained. Be this as it may,

there can be no doubt that the reopening of this question is, in in a great measure, due to the fact that the success attained by other methods has been such as to cast doubts upon the assumed superiority of the lateral operation. The splendid results obtained by Cheselden, Crichton, Liston, and above all by Martineau, have not by any means been achieved by operators generally. The success in the lateral operation seems to depend a good deal on an uncommunicable knack—a sort of instinctive appreciation of the proper amount of work to be accomplished by the knife—an ability to measure accurately the force to be employed. As this can be neither taught nor acquired by rule, it has happened that the best surgeons and anatomists have often proved the most unfortunate in the practice of the lateral operation. The results of this method, have been very unequal; by it have been obtained the *highest individual success*, and the *lowest general averages*.

As this fact has forced itself on the attention of surgeons, many modifications of the operation have, from time to time, been suggested, some of which are now beginning to be adopted; others, after having been prematurely lauded, have fallen into neglect.

There has never been much diversity of opinion about the *principles* of perineal lithotomy. Two points have been admitted by most surgeons. First, the necessity of an external wound, so situated, and of such an extent, as to afford free access to that part of the urethra to be opened, and allow ready escape of urine and other discharges without risking important structures. Secondly, an internal or prostatic wound, not larger than is absolutely necessary to permit the extraction of the stone without undue violence.

The exact position, form, and extent of the external opening has justly been looked upon as a matter of secondary importance. And so it is in itself; but the advantages of having direct and free access to the membranous portion of the urethra, for the purpose of rendering the subsequent steps in the operation more easy and precise, has, until recently, been rather overlooked.

What size of internal wound suffices to fulfil the admitted condition has given rise to much discussion. Thus, we find the late Professor Syme, in 1842, stating that he considered it necessary to "cut freely through the whole thickness of the prostate, in order to obtain room for extracting an ordinary calculus;" and he said that he had never seen any bad consequences follow this practice. While, in the succeeding year, Fergusson advocated the principle of limited internal incisions; and, so far from *cutting through*, advised that only a *slight notch* be made into the left lobe of the prostate.

That the advocates of smaller incisions frequently made wounds *unintentionally*, not much different from those advocated by Professor Syme, is very probable.

Mr. Bryan, a London surgeon, writing about this time in the Lancet, says:—"I examined, at every opportunity I had, the wound made in those subjects which had been used in exemplifying to surgical classes the manner of operating. In every case which I met with, I found the prostate completely divided, and the vesiculæ sliced into; although the incisions had evidently been made according to rules taught in the schools of surgery."

Those who believed that complete section of the prostate was the smallest wound, fulfilling all the requirements of the case, as well as those who aimed only at notching the gland, all agreed as to the desirability of rendering the wound as exact as possible, both as regards site and extent. Although generally content with the ordinary lithotomy staff and knife, doubts seem now and again to have arisen in the minds of some, belonging to either party, whether the desired degree of exactness could be attained by the ordinary instruments used in the ordinary way. Thus, we find Syme, in 1844, describing a peculiar knife called a prostatome, but he does not seem to have long continued its use. He also used, for a time, a straight guide upon which to make the prostatic wound. About the same time, or shortly before, Sir William Fergusson recommended a knife peculiar for the narrowness of its blade.

With the same object in view—viz., that of securing an internal opening of definite size, Sir Philip Crampton, in operating, made the deep wound with "a narrow knife, blunt for half an inch from the point," which lay concealed within the groove of the staff. After pushing this into the bladder, he removed the staff, and, lateralizing the knife, passed his forefinger along its back into the bladder. He remarked that, by this means, "the opening into the bladder is, in all cases, and without reference to the prostate, exactly sufficient to receive the forefinger of the operator, plus the blade of the bistoury." The importance attached to this point in the operation is further evinced by the interest taken at the time in the discussion about the way in which Liston held his knife—whether "overhand" or "underhand?" It was simply a question whether he held it in such a manner as to make an indefinite thrust into the prostate, or so as to be better able to guide and control the advance of the blade along the groove in the staff.

In 1844, Sir John Fife operated by a method which seems to have been precisely similar to the medio-lateral operation

lately introduced by Sir William Fergusson. Two years later this same mode of operating was practised with very good results in the Manchester Infirmary by Mr. Ransome; but it did not at that time receive much attention from the profession.

In the year 1848, another, and what has proved a most important effort towards improving and simplifying lithotomy, was made by Professor Buchanan, of Glasgow, who then described his mesial operation with the rectangular staff, the mode of performing which I need not detail. He claimed for it the following advantages: ease of performance, a small incision into the prostate of definite size, with less risk of hemorrhage and deep-seated infiltrations. This operation, practised with a success that has compelled respectful attention, must be recognised as the first successful effort at median lithotomy. There are men who condemn all attempts at improved modes of operating with new instruments as dangerous devices to make up for deficiency of surgical skill. Such men had nothing good to say of what was then somewhat contemptuously styled the "rectangular operation."

We may just notice, in passing, that Dr. Alfred Post, of New York, Dr. Warren, Dr. Steven, and others in America, were about the same time advocating and practising modifications of the bilateral. Dr. Warren adopted the Celsian external incision, opened the membranous part of the urethra in front of the prostate, and after introducing into the bladder a probe-pointed knife, having a long handle, he withdrew the staff. He then directed the knife, supported by the forefinger of the left hand, first towards the left tuberosity of the ischium, then towards the right, cutting the prostate to the extent of three-fourths of an inch in either side. He advocated this operation on the following grounds:—1st, The parts cut are more simple; the nerves and vessels being smaller, the pain and hemorrhage are less. 2nd, The access to the urethra is more direct. 3rd, The prostate can be cut with great precision. 4th, The opening can be made double what can be attained by the lateral, without transgressing the limits of safety. The advantages of the external semilunar incision are thus clearly recognised and stated—directness of access to the urethra allowing of greater precision in the prostatic incision.

The result of Dr. Buchanan's ingenuity and success was evident in the renewed attention paid to median lithotomy. In the year 1850, Fergusson recorded a case in which he operated by a X-shaped external incision; as suggested by Professor Eve, of Nashville. He has stated lately that from this time, although he continued to teach and practise the

lateral operation, his leaning towards the central part of the perinæum had been leading him, instead of beginning his incisions on the left side of the raphe, to venture more and more to the right.

Allarton's median operation was first described in 1854. In this operation the prostate and neck of bladder are left untouched by the knife, and dilatation pure and simple is made to take the place of incision. Many surgeons, both in London and in the provinces, gave this operation a trial; and various instruments were invented for effecting dilatation.

The mortality in the patients operated on by this method contrasts favourably with that in the lateral operation; but the impression obtained by a study of the reported cases is, that many surgeons judiciously restricted its employment to those cases in which the stone was small, and in which dilatation would suffice for its removal. The greater number of the cases of death after this method have just been when dilatation equivalent to laceration had to be practised in order to extract a large calculus. One thing is certain, that this operation paid little respect to that structure demonstrated by Liston and others, to which the late Professor Syme directed attention and attached great importance,—"the sensitive ring which surrounds the neck of the bladder, at the base of the prostate in the male, and at the corresponding part in the female, which forms an obstacle admitting of ready removal by incision, and cannot be overcome by tearing without almost certain death."

The results of Allarton's median lithotomy, as well as Willis's operation of lithectasy, proved that the danger from forcible dilatation, although real and considerable, had been exaggerated.

Allarton's medial operation differs from lithectasy as proposed by Willis, inasmuch as the former effects by rapid, what the other accomplishes by slow, dilatation. Slow dilatation is safe enough when applied to the intact female urethra, but following and affecting a wound, it is both painful and dangerous. The danger in the case of lithectasy arises from the continued irritation of slow and increasing tension acting on parts particularly disposed to inflammatory action from having a wound on the one side and a foreign body on the other. Rapid dilatation, again, although harmless when not carried beyond a certain point, becomes dangerous when it exceeds that limit. It is probable that, if the urethra be dilated much beyond one-third of an inch, the process then becomes another name for laceration; and when a large stone is dragged through a wound so dilated, bruising follows laceration. Such injuries cannot be treated lightly, especially when, as in the

ease of lithotomy, they involve large venous plexuses and friable glandular tissues, exposed to the passage of irritating fluids, and at the entrance of a viscus having extensive sympathetic connexions. Experience has confirmed what might have been guessed *a priori*, that death, when it occurs after such operations, is due to violence. The fact that the fatal results are not more common after such procedures, is confirmation of the truth of the observation, that laceration and bruising, with limited incisions, although far from harmless, are less fatal than an excess of cutting.

The late Mr. Lloyd's recto-urethral operation is also a median one, and is the same as Allarton's, in so far as the treatment of the prostate is concerned. It has one advantage, however, over Allarton's, for it gives a freer external wound, thus facilitating the dilatation as well as the subsequent extraction. The wound in the rectum communicating with the urethra, not with the bladder, readily heals, as I can myself testify. He continued to use this operation for many years with excellent results, but it has never met with much favour from the profession generally.

We have already observed that various operations, which may be called medio-lateral ones, had been practised by different surgeons years ago. These, if we except Dr. Buchanan's, never attracted much attention. In 1867, Sir William Fergusson operated on a boy by an external semilunar incision, making the deep wound precisely as in the lateral operation. He lays it down as a rule, that "the deeper the wound, the more difficult and dangerous the operation," particularly in respect to the seizing and extracting of the stone, and insists that the lunated incision gives a much more extensive, patent, and direct external opening than any straight line could afford. The reports of cases treated by this operation by so eminent a surgeon, and who had all along been one of the most illustrious advocates of the lateral operation, and who had practised it with acknowledged success, directed the attention of the profession to this subject, and brought out the fact that several of the leading surgeons had already given up the exclusive use of the lateral operation.

Thus, Erichsen writes in the Lancet, 11th January, 1868:— "Although practising, as a rule, lateral lithotomy by Cheselden's method, as modified by Liston, I have for a long time had grave doubts whether it was really the best way of getting into the bladder, and especially of extracting a large stone out of it." He prefers Dupuytren's operation, but performed with a rectangular staff. The external wound is semilunar, but he makes the deep wound with a lithotome caché, cutting equally into both lateral lobes of the prostate.

In the same way, Sir Henry Thompson informs us that he also has given up the exclusive use of the lateral. He practises Civiale's operation, the essential features of which are a vertical mesial external incision, as in Allarton's, with a deep incision similar to that practised by Erichsen, made with a similar instrument. Hutchison, who, so early as 1857, had made use of a rectangular catheter staff, which was simply a modification of Buchanan's, reported that he had for some time operated by various forms of the double-cutting gorget, and with a mesial incision.

It is thus evident that the lateral operation is no longer held in the same esteem that it once was. No doubt, there are fashions in surgical incisions and operations just as in other things; but the growing opinion in favour of the semilunar incision cannot be so accounted for. The men who have declared in its favour are men who are well able to compare and contrast it with the lateral incision. The same remarks apply to the pretty general adoption of the rectangular staff.

Before the publication of these innovations, I had abandoned the lateral in favour of an operation which I shall now describe. The staff I use is simply a modification of Buchanan's rectangular one, the acute angle being replaced by a gentle curve, and grooved on its inner aspect. I may just remark, in passing, that Buchanan's staff, as modified, is an excellent instrument for performing any of the ordinary operations. It forms a useful guide to that part of the canal which is to be opened, conducts straight into the bladder, without endangering the rectum. The form of the external wound in the operation I am now describing in semilunar, curved somewhat lower and deeper upon the left side than the right, in order to afford a freer drain on the side on which the prostatic wound is situated.

Having exposed the membranous part of the urethra, this is opened, and a guide is introduced along the groove in the staff into the bladder. This guide, consisting of two parallel and connected bars or blades, capable of being separated by means of a screw, forms, when approximated, an instrument about five inches long, somewhat like a female catheter in size, straight, slightly flattened from above downwards, and grooved on the left side so as to permit the bottom point of the knife to slide along without escaping from it. When this guide has been fairly introduced into the bladder the staff is withdrawn, and the blades are separated by the screw *to such an extent as to render the tissues tense.* The knife used has a button-shaped extremity to fit the groove in the guide. The blade is narrow throughout, but is slightly triangular, becoming a little broader

towards the handle than at the point. This knife is then carried along the groove so as to divide the tense resisting structures, *by its edge being brought into contact with them rather than by actual cutting.* The blades of the guide are now expanded to a sufficient extent. Should it be found, however, that the required degree of separation cannot be effected without force, the knife may be passed along a second time to cut the structures that still resist. In no case should anything be incised except what is found to oppose gentle dilatation. The size of the calculus determines the extent to which the knife is to be used and dilatation effected.

The blades of the instrument having now been separated so far as the size of the stone seems to demand, it serves as a conductor for the forceps into the bladder. They slip along between the blades of the instrument. The stone is then extracted in the usual way. Should the calculus be very large, the knife may be applied in precisely the same manner to the right side of the prostate, thus making a bilateral incision. This is seldom required.

The chief advantages of this, as compared with other perineal operations, are the following:—In the lateral operation, the incision passes indefinitely into the prostate, sometimes dividing it entirely, and at other times only notching it, and this *quite apart from the purpose of the operator.*

There is thus a want of exactness as regards the extent of the wound in the prostate. Then there is no less a want of discrimination as regards the structures cut; those that require incision, in order to permit easy dilatation, being sometimes left intact, while as frequently the knife passes indefinitely into or through the prostate without reference to resistance. Further, the incision bears no proportion to the size of calculus to be extracted. Mr. Oliver Pemberton, in his Address on Surgery at the last meeting of the British Medical Association, notices this point, and says:—"I hold that it cannot be necessary to make incisions to the same extent to remove a body the size of a pea as we should make for one the size of twenty." I hold so too; but, in lateral lithotomy, this is practically the case. A similar vice, however, lies at the root of Allarton's operation, which Mr. Pemberton has adopted instead of the lateral. It treats all stones, whatever may be their size, in the same way; and if the stone be large, and the tissues tense, then more force must be applied.

In the lateral operation, the manual dilatation which follows the incision has reference to the admission of the forceps; the subsequent dilatation, which is practised in the act of extraction, is the complement of the incision. The extent to which this is carried, and the force required to effect it, vary accord-

ing to the size of the calculus. In the median operation, laceration and bruising are inevitable when the stone is large. The same holds true of the lateral, when merely the apex of the prostate has been notched.

In the operation I have described, the incision and dilatation are in proportion to the size of the stone and the resistance of tissues.

Mr. Martineau of Norwich, who cut eighty-four patients, with only two deaths, has been reckoned amongst the advocates of large incisions. More truly he may be regarded as an advocate for *discriminating* incisions—incisions bearing reference to the size of the stone—incisions also bearing special reference to the *resistance of tissues*. He says:—"Should the stone be large, or there be any difficulty in the operation, rather than use much force, while the forceps have a firm hold of the stone, I give the handles to the assistant, who is to draw them outwards and upwards while the part forming the stricture is cut; which is easily done, as the broad part of the blade becomes a director to the knife; and rather than lacerate, I have often repeated this enlargement of the inner wound two or three times." I have little doubt that this practice of incising what resisted, and in proportion to the resistance and to the size of the stone, explains his exceptional success. One of the most difficult parts of the operation, or it may be one of the most dangerous, even when not difficult, is the introduction of the forceps. The neck of the bladder is apt to be somewhat rudely disturbed in this part of the operation, more especially if the wound be deeply situated, and just sufficient to admit the forefinger. Remove the forefinger, and the wound contracts, so that the forceps are more likely to push up the neck of the bladder than enter it. Introduce the forceps along with the finger, then the forceps and finger jointly produce a considerable degree of bruising. To introduce the forceps *while in the act* of removing the finger is a good precept, but somewhat difficult to follow out. In this operation the guide for the knife acts as a conductor for the forceps at the same time.

Some of the special accidents of lateral lithotomy are less likely to occur in this operation. Troublesome or even fatal hemorrhage is one of the worst of these. One in eight cases of death after lateral lithotomy is due to this cause. It has occurred to such men as Sir Charles Bell in England, Klein in Germany, and Dr. Physic in America. Mr. Pemberton of Birmingham relates such a case in his practice, in which death followed after twenty-four hours. The patient was a boy. This surgeon remarks:—"I could find nothing in the post-mortem to account for the bleeding. . . . . It was clear the cutting did it somewhere, and I naturally thought I should like, in the

future, to extract such a stone as the one I then removed with less of the cutting in the dark." In operating as I have described, from the open nature of the wound and its situation, dangers of cutting in the dark are prevented. Inflammation of the neck of the bladder and pyæmia caused by bruising, and infiltration of urine and pelvic abscesses, the result of too extensive incisions, are alike avoided.

Statistics.—The number of cases operated on in the Antananarivo Hospital by this method since the arrival of my colleagues, Drs. Borchgrevink and Mackie, is 32. Of these, 11 were operated on by myself, and 21 by my colleagues. One of my cases was followed by a fatal result about a month after the operation, and a few days after the patient had actually left the hospital. A post-mortem examination revealed extensive disease of both kidneys. There were several stones an abscesses in the right kidney; the left kidney also contained a few small calculi, and was excessively engorged. The prostatic wound had completely healed, but the external wound was not entirely cicatrized. Of the 21 cases operated on by Drs. Borchgrevink and Mackie, 13 had occurred before I left the island fifteen months ago; and 4 were still in hospital when I heard last, and were reported as progressing favourably. One death occurred amongst the 13 operated on before I left. He was one of Dr. Mackie's patients. No post-mortem examination was obtained, but the symptoms pointed to kidney disease. I am not able to give at present exact details of the ages of the patients and the size of the calculi in these cases. I hope to publish these particulars on a future occasion.

The cases I operated on myself were most of them very favourable ones for the operation, the patients being all under puberty, and the stones of moderate size. The largest stone that I have extracted *by this operation* weighed 2 oz. and 3 i., from a boy, aged 5 years, who had suffered from the symptoms of calculus ever since he had been a year old.

The patients operated on by my colleagues were some of them adults. I believe I am safe in saying, that none of the stones extracted in the 13 cases operated on while I was there weighed above 2 oz.

In one of the cases that occurred after I left, the stone extracted weighed 1590 grains, *i.e.*, considerably above 3¼ oz.

Were the object in view to *contribute* to the already superabundant mass of lithotomy statistics, the four cases (the final result in which may be looked upon as more or less doubtful) would have to be eliminated, and, according to the principles upon which statistics are compiled, the fatal case which occurred in my practice, since it took place after the patient had left the hospital, would also be left out of the calculation,

giving a mortality of 1 case in 28. But there is no sufficient reason to exclude this case, as the object of all operations is the lengthening of the life of the patient. In this case I have little doubt that the operation shortened his life to some extent. The general result—1 in 16—attained by this operation, I consider, has been above the average, even admitting, as I do, that the cases were upon the whole more favourable for the operation than those met with in hospital practice at home.—*Edinburgh Medical Journal, June* 1873, *p.* 1057.

62.—ON URETHROTOMY OR LITHOTRITY IN AGED AND DEBILITATED PATIENTS.

By W. L. CROWTHER, Esq., Tasmania.

In the Lancet of the 8th March appeared the report of a clinical lecture delivered by Sir Henry Thompson at University College Hospital, and it is stated in some editorial remarks in the same journal such lecture "represents that distinguished surgeon's views upon a most important question." It is asked whether, in the presence of a calculus, chronic degeneration of the kidney can in all cases be diagnosed; and, if so, what is the best treatment to be adopted under the circumstances?

Before entering upon this subject in detail, I may say it is not my intention to dispute, even were I able to do so, Sir Henry Thompson's views upon a subject upon which he is so well entitled to speak with authority; nor am I prepared to say aught that would lessen the value and importance of lithotrity as a scientific operation as performed by such a practised hand as that of Sir Henry himself, nor gainsay the fact that there are numerous cases to which it is applicable; but to direct the attention of the profession to the circumstance that there is an operation infinitely safer than lithotrity for the class of cases to which Sir Henry refers, and which, in my humble opinion, gives the only chance for, if not restoration to health, at least prolongation of life—I mean urethrotomy by the median section, with simple dilatation by the finger of the prostate gland. My experience as an operator must of necessity be vastly inferior to that of Sir Henry Thompson and other metropolitan celebrities; but as it has fallen to my lot during the last thirty years, amongst many others, to deal with a few of the class of cases to which reference has been made, I should not be performing my duty either to the profession or the public without making known the result. I believe—and all surgeons of any experience must be agreed upon the point—that there is no certain means, especially in cases of calculi of large size, of determining with accuracy the actual amount of organic lesion to which the kidney or bladder, or both, have been sub-

jected; or, in other words, what there is left, were the calculus removed, as a rallying point to the feeble powers of life. The ordinary tests, chemical and microscopical, give indications of a very reliable character, by which, coupled with the history of the patient, his state of health and mode of life antecedently to the formation of calculus, and more particularly the effect upon the constitution subsequently to such formation, a diagnosis may be made sufficiently accurate to at least warn the patient and his friends of the danger attendant upon any operation, however well executed, and what the chances may be of success; and the question to be answered is, By what treatment will that danger be least? To that question I am prepared to reply.

I have no data before me other than those supplied by Sir Henry Thompson's lecture, and another case of his, that of the Emperor Napoleon, as to the amount of shock to which the system is subjected by the crushing process, but these will be sufficient for my purpose. Of the four cases two recovered. The second had "*five* sittings in ten weeks," and, *although handled with the utmost caution*, had "prolonged fever, and severe rigors following some of them," and was, for a time at least, in imminent danger. In the third "*five* sittings sufficed to remove *nearly all* the stone; but the last was followed by *severe shivering and vomiting*, and death in a few days." The fourth, that of the Emperor Napoleon, although his condition was to a certainty better than any of the preceding, exhibited precisely similar symptoms, and sank after the third operation, *half the stone only having been dealt with*. Sir Henry Thompson, in his work on "Lithotomy and Lithotrity," makes the following significant remark:—"But a little reflection may suffice to convince us that in all cases it is unwise to cut into, *and still more so to crush* in, the cavity of a bladder which is the subject of unchecked chronic inflammation, as almost all bladders are, in a greater or less degree. In such a condition a very little interference will often set up acute cystitis, and from this source morbid action may invade the kidneys and prove rapidly fatal." And in the leading article referred to the question is asked, "What is to be done when a patient is worn down by long-continued sufferings?" The answer, based upon the opinion just quoted, were there none other, is a simple one: Select for the removal of the foreign occupant from the bladder the operation that will produce the slightest shock, and that *only a single one*, to the nervous system, at once relieves the bladder and constitution of all irritation, involves no cutting either of the prostate gland or bladder itself, and in its performance is attended *in most cases* with the smallest loss of blood, is completed in one or two minutes, entails no risk of

subsequent hemorrhage, and makes but little demand upon the powers of life for its reparation. That operation, *applicable alike to stones of all sizes*, is the one to which I have previously drawn attention—"urethrotomy by the median section with dilatation of the prostate gland." Mr. Erichsen in 1859 drew attention in the columns of the Lancet to the benefit of the "median operation" with prostatic dilatation, but limited it to stones of small size or such as would now be dealt with by lithotrity. Long before his paper appeared, and adopting the mode of section proposed by Allarton, I had made it applicable to such cases as those referred to by Sir Henry Thompson, believing the safety of the patient, whether young or old, and particularly the latter, was greatly enhanced by the incision passing through structures that were in the main "aponeurotic," and from the restricted use of the knife in the deeper parts the *grave perils of the ordinary lateral operation were wholly avoided.*

Appended are the notes of a case in support of the opinion, if such were wanting, that the shock to the system by this operation is slighter than in any other, and the recovery more certain. I have long been of opinion that the defects inherent in the crushing process in aged people, more particularly when the calculi are above the "medium" size, and whose bladders are in a high state of irritation and inflammation, such as the Emperor Napoleon's was proved to have been prior to the first operation—"urine bloody and purulent,"—arise not only from the direct mechanical embarrassment to which the bladder is subjected at each sitting, but that the shock to the system of any single crushing is equal to, if not far in excess, of that arising from the operation I have been in the habit of performing successfully; and I must be pardoned if I give offence by expressing the conviction—but it is forced upon me, and that by the reasoning of Sir Henry Thompson himself—that in the Emperor's case the system would have borne the single shock of the median section with non-incision of the prostate, but could not bear up, in the presence of so large an amount of organic disease, against the repeated assaults upon it necessary to rid the bladder of a stone of such large proportions as the post-mortem examination revealed the Emperor's to have been.

Samuel H., aged seventy-five, has for the last two years had well-marked symptoms of vesical calculus, but until a short time since did not submit to an exploration of the bladder. Is extremely irritable, and exhibits a great dread of any operation. Health and constitution much broken; countenance pallid and waxy; and the sounds of the heart give evidence of extensive atheromatous deposit. The urine is bloody and muciform, and under the microscope exhibits tube-casts and pus-globules; sp. gr. 1020.

April 9th, 1873. 12.15 p.m.: After having been placed under the influence of chloroform, "urethrotomy" by the median section was performed. The time occupied in completing the operation was two minutes. The loss of blood did not exceed one ounce. Calculus lithic-acid.—5 p.m.: Pulse firm. Has slept at intervals. There has been a copious flow of urine, and slight venous oozing from the wound.—10 p.m.: No constitutional disturbance. Has taken diluents and broth freely; and, beyond a little scalding during the transit of urine, has no uneasiness.

10th. 10 a.m.: Has had a good night. Urine clear, and passed through the wound at intervals only.—5 p.m.: Has full control over the bladder, making water into a bed-pan as occasion demands. Tongue moist and clean. No febrile disturbance.

11th. 7 a.m.: Bowels relieved by an enema. Wound contracting rapidly.—8 p.m.: Has risen from his bed every two hours to empty the bladder, urine passing each time both by the wound and urethra. Slept during the day. Says he is quite comfortable. Tongue moist; no thirst.

12th. Has not passed any water through the wound since 10 p.m. last evening. Empties the bladder every three hours. 6 p.m.: Continues the same.

13th. Complains of pain in the urethra during the passage of urine, and appears to have a little uneasiness about the neck of the bladder, with increased desire to micturate. Ordered a warm hip-bath and domestic enema. Incision contracted to a mere line.—6 p.m.: Had perfect relief by the warm-bath. Has been up in a chair for two hours, and is gaining ground rapidly.

14th. 7 a.m.: Has slept well. Wound closed. Bladder emptied by the natural passage every three hours during the night, and without pain.

15th. Sat up for four hours. Has no uneasiness, and is now convalescent.

17th. Rose early this morning, and, at the time of my visit, has dressed and walked about the house.

In this case, after the first twenty-four hours, the patient had full control over the bladder, and did not soil his bed; and, as soon as the effects of the chloroform had passed away, gave no evidence of either mental or physical embarrassment. In some of the other cases, with much larger calculi, the recovery has been equally rapid, and nearly all were marked by an absence of any important local or constitutional symptoms.—*Lancet, Nov.* 1, 1873, *p.* 624.

[Mr. W. F. TEEVAN does not agree with Mr. Crowther in his views about the prostate. He says Mr. Crowther thus explains his method:—"I mean urethrotomy by the median section, with simple dilatation by the finger of the prostate gland."]

Now, Professor Ellis has, years ago, shown that the prostate could not be dilated, and I would recommend any surgeon who has a doubt on the point to attend his incontrovertible demonstration on the subject. To say more would almost seem superfluous, but it may not perhaps be remembered that the prostate gland is encased in two jackets of white fibrous tissue, so dense and strong that the capsule is described, in Quain and Sharpey's Anatomy, fifth edition, page 1216, as "rather difficult either to tear or cut;" and yet this coat enveloping the prostate is supposed by many surgeons to stretch like a piece of india-rubber to kindly allow them to take out the stone and then to quietly return to its pristine condition. But facts prove that the prostate has never dilated for any surgeon, for it has only given way when lacerated or cut. Why has Dolbeau introduced perineal lithotrity? Simply because he found, by experiment on the dead subject, that the prostatic urethra could not be dilated beyond twenty millimetres, which is about its normal diameter in old men. The experiments which I myself performed in reference to the subject proved that the prostate will only admit the terminal joint of the forefinger without lacerating, and a personal examination of all the specimens in the various museums in London a few years ago, showed me that in no single instance had a stone been removed from the bladder without lacerating or cutting the gland. Mr. Crowther refers to a lecture of Mr. Erichsen's; but, unless I am very much mistaken, that gentleman has considerably modified his views, and believes that in practical lithotomy, small calculi excepted, a stone must either be cut out or torn out. The objection to the method of so-called dilatation is, that it involves a laceration or bruising of the plexus of veins surrounding the prostate, and hence the common cause of death after lithotomy is pyæmia; and the procedure has, moreover, to answer for those disasters, by no means few in number, which have befallen lithotomists. Those surgeons who extract calculi by the so-called process of dilatation, which is in reality laceration, lose far more patients than those operators who cut rather than tear. No observation can be passed on the single instance related by Mr. Crowther, as he has omitted, quite accidentally I should say, to state what was the size of the stone he removed; nor can any remark be made on the Emperor Napoleon's case, as the details are still withheld from the profession. —*Lancet, Nov.* 8, 1873, *p.* 683.

63.—ON SUBCUTANEOUS URETHROTOMY IN STRICTURES OF THE URETHRA.

By W. F. TEEVAN, Esq., B.A., Surgeon to the West London Hospital, and to St. Peter's Hospital.

The operation of subcutaneous urethrotomy is little known or practised in this country, but it is the one which strongly recommends itself to us for adoption whenever an operation may be required. It may be safely said that if a stricture be large enough to admit of the introduction of a dilator or urethrotome, you can cure the complaint by gradual dilatation without the slightest risk to the patient's life. There are, however, a few cases where, although the stricture may be dilated to the fullest extent, it yet so rapidly contracts again that an operation is indicated; and there are also instances of hard, horny strictures, complicated with abscesses and fistulæ, where an operation is called for, as much for the purpose of giving a free vent to suppurating tissues as for the enlargement of the canal to its natural calibre. In the first case, subcutaneous urethrotomy is indicated, but in the latter instances external urethrotomy. The subcutaneous division of strictures, introduced into practice by Dr. Henry Dick, is the only operation which holds out any prospect of a real cure for stricture, as it is the only method that is in unison with the teachings of surgical pathology and experience; it possesses all the advantages of external urethrotomy or perineal section without the disadvantages of an external wound, and it is superior to internal urethrotomy because it always insures a complete division of the strictured parts, which that operation does not.

J. N., a strong healthy sailor, aged 52, came to St. Peter's Hospital, as an out-patient, on June 1, 1872. He stated that he had suffered from strictures for upwards of twenty years, and that eight years ago he had been operated on at an hospital in London, where his stricture were forcibly split, and an instrument regularly passed for some time afterwards. The complaint, however, returned as bad as ever, and one year and a half ago he visited the hospital where he had previously been, and submitted to a repetition of the former operation, a catheter being regularly introduced for some weeks afterwards. In the meantime the patient had learnt to pass the instrument himself, and used it twice every week till he was forced to desist through the occurrence of some heavy weather at sea. After an interval of ten days he again tried to pass the catheter, but failed, as the strictures had in the meantime contracted. When the patient came under my care I could only

pass a No. 3 bougie, and that with considerable difficulty. Examination showed that there were two strictures encircling the urethra like iron rings. The distal stricture was half an inch broad and four inches from the orifice; whilst the other one was a quarter of an inch broad and two inches and three-quarters down the urethra. By the end of the month the urethra had been enlarged by gradual dilatation to No. 9; but when the patient came to the hospital, ten days later, the strictures were found to have again contracted, and would only admit No. 4. He was then admitted into the hospital, and a No. 4 elastic catheter was passed into the bladder and left there for an hour, when it was replaced by a larger instrument. By continuing this method of treatment for six hours, the patient's urethra was dilated to a No. 8, when the patient could no longer persevere, on account of the increasing pain. Next day the strictures were again found to have contracted, so that only a small instrument could be introduced. It was now clear that an operation was called for, and accordingly, on July 26, in the presence of Dr. Henry Dick, Dr. Green, and Dr. Magrath, I passed a fine straight staff through the strictures, and completely divided both of them, subcutaneously, with a small tenotome, which I introduced beneath the skin by making a small puncture. The contracted tissues were so dense and hard that their division was attended with a creaking noise plainly audible to those present. A very large catheter was then introduced into the bladder and left there for half an hour. Blood oozed away from the urethra for ten minutes after the operation, but none escaped through the valvular puncture made by the tenotome. The patient's urine was drawn off the same evening and three times the next day. On the third day the use of the catheter was relegated to the patient, who passed it once a day till he left the hospital, on July 31—five days after the operation—not having had a single bad symptom, nor having been confined to bed. The patient came to the hospital on September 2, to report himself before sailing. He was then passing No. 14 elastic catheter with ease twice a week, and was in all respects quite well. The ends of the divided strictures could be plainly felt beneath the skin united by bands of supple and non-indurated tissue.

The strictures were in the first instance the result of a long-continued gleet, and the splitting (lacerating) operations to which this man had been subjected had only given temporary and delusive relief, for each operation had been followed by a more speedy return of the complaint. One point of great interest was that the patient had had his strictures treated thirty years ago on board the *Dreadnought* by the system of gradual dilatation. It would thus appear that the results of

that treatment were more abiding than those produced by immediate dilatation, which was in reality complete rupture or laceration. The strictures became converted into traumatic ones by the surgical lacerations to which this patient had been subjected; for such strictures as his, uncomplicated as they were with abscesses or fistulæ, were rarely seen unless produced by accidental or surgical injury. No two operations were so dissimilar in character as forcible dilatation and subcutaneous urethotomy, for the former was an exhibition of blind force, whilst the latter was a demonstration of applied skill.—*Med. Times and Gazette, Sept.* 27, 1873, *p.* 351.

64.—DESCRIPTION OF A GROOVED CATHETER WITH CONDUCTING BOUGIE, WITH REMARKS ON ITS VALUE.

By W. F. TEEVAN, Esq., B.A., Surgeon to the West London Hospital, and to St. Peter's Hospital.

The instrument I am about to describe is one of the most useful for the treatment of impassable stricture, retention of urine from stricture, and for effecting an entry into the bladder in those cases where the difficulty is caused not so much by the narrowness of the canal as its deformity (Delpech), a condition that is but little recognised in this country, and which I may best describe by saying that it means a crookedness of the urethra, caused by constrictions and the dilatations resulting therefrom, and necessarily occurring posteriorly to such strictured points. The idea of sliding one instrument over the other is by no means new, and originated, I believe, with Nauche, in 1816, and was employed by Civiale in 1823. The method has also been well known for a great many years to M. Auguste Mercier. The Americans would seem to be ahead of us on this point, for they have for some time possessed a most beautiful and useful instrument, designed, I believe, by Professor Gouley, of New York. In this country the method of treating stricture by sliding one instrument over another has been prominently brought before the profession by Mr. Thomas Wakley, but has not received that attention which its usefulness deserves. I am informed that the late Mr. Solly employed an elastic catheter open at both ends, which he slid along a catgut bougie, and I have lately seen this plan tried by my colleague, Mr. Walter Coulson; but I consider it open to objection, for the catgut bougie is soon affected by the moisture of the canal, becomes twisted and knotted, and thus makes it difficult, and sometimes impossible, to slide a catheter over it. Apart from the gentlemen whose names I have men-

tioned, I believe that the method is but little known in this country. I would premise my observations by saying that the whalebone bougie is really the invaluable portion of the compound instrument I am about to describe. It often happens that no silver or elastic catheter can be got into the bladder, and yet a fine bougie may be passed with ease, and the retention relieved. Such a case I have recorded in the Transactions of the Clinical Society (see vol. iv., p. 124). Having succeeded in getting such an instrument into the bladder, there are often reasons which make it desirable or necessary that we should pass another kind of instrument, either for operation or dilatation, and it therefore becomes of the highest importance to utilise the bougie that has been passed into the bladder, and this is best done by retaining it *in situ* till we have been enabled to slide a larger instrument over it. I would, indeed, go further, and say that in some cases a fine bougie is the only instrument which can be got into the bladder, and that unless it is utilised we cannot proceed with the treatment of the case. Again, there are cases, complicated with false passages, where no instrument can be introduced except a bougie with some peculiar twist, and where our only chance of progress lies in enlarging the canal by sliding one instrument over another.

The instrument I am in the habit of using differs in some respects from Professor Gouley's, and is, I think, superior to it. I will first describe it, and then show the differences between the two. The conducting bougie is made of whalebone, and is excessively fine, having each end olive-shaped, which prevents the instrument from pricking the mucous membrane or lodging in the lacunæ. The material of which the bougie is composed imparts great elasticity combined with firmness, and thus admirably fits it for getting through very indurated strictures where the entrance is often reduced to a pin's point. Care ought to be taken that the bougie is at least two inches longer than the catheter, otherwise the former may recede insensibly from the bladder as it is held forcibly between the thumb and forefinger of the left hand to resist the tendency which the catheter has to carry the bougie with it as it is sliding over it into the bladder. The catheter is about the size of a No. 1, English, and its end is cut off to permit the bougie to enter the canal and emerge through a slit, which serves as a groove for subcutaneous or external urethrotomy. The groove is made by cutting out a longitudinal piece of the catheter, Urine will not escape through it, for the mucous membrane is tightly stretched across it, and so completely closes the groove. The three differences between Professor Gouley's instrument and the one I have described, are:—1.

Mine is much smaller. 2. The groove is made by cutting a piece out of the catheter, but in Professor Gouley's it is made by driving in the wall of the catheter, thus necessarily diminishing the calibre of the canal. This explains how it is that a smaller instrument can be constructed after my plan than Professor Gouley's. The bougie in my instrument is contained for about one inch and a half in the canal, but in Professor Gouley's it is only passed through a little bridge at the end, not more than one-eighth of an inch broad, and is thus far more likely to become entwined round the catheter than when it passes through a considerable portion of the instrument. Any surgeon can have one of his small catheters converted into the one I have described for an insignificant sum, and so be spared the expense of buying a new instrument. In introducing the bougie into the bladder, it is well to rotate the instrument as we proceed, for by that procedure we avoid all chance of slipping into the lacunæ on the way. It sometimes happens that, although we can pass the bougie through the stricture, we are unable to make the catheter pass along it, on account of the narrowness of the strictured part. In such a case it is well to leave the bougie in the bladder for several hours, and then try again. The compound instrument I have described will be found most useful, (1) for the relief of retention; (2) for the purpose of dilating the stricture; (3) for external urethrotomy; and, lastly, for subcutaneous urethrotomy.—*Lancet, July* 5, 1873, *p.* 5.

65.—ON TWO CASES OF DEATH AFTER HOLT'S OPERATION.

By Dr. R. W. TIBBITS, Surgeon to the Bristol Royal Infirmary.

[The two cases related in the following paper throw considerable doubt upon the absolute correctness of Mr. Holt's statement that the use of his instrument is "free from every danger —immediate or remote."]

The first case was that of a healthy-looking sailor, aged 25, who was admitted into No. 11 ward on February 21. Five years ago he had been laid up with a sharp attack of gonorrhœa, and since then had suffered from a gradually increasing stricture. Several unsuccessful attempts had been made by medical men to pass instruments into his bladder; after each attempt he had shivered and felt unwell for one or two days. He was kept in bed for three days, the bowels freely opened with castor oil, and on the evening of February 24 a full dose of Dover's powder was administered.

February 25. Holt's instrument was easily passed, and the stricture—a slight one—split with the largest-sized dilator. Afterwards No. 11 catheter was introduced into the bladder.

26th. Patient seemed perfectly well. Had not shivered after the operation, and passed his urine in a full stream, with slight pain.

27th. Still going on well. No. 10 catheter passed easily, causing little pain.

28th. Twenty-two hours after the passing of No. 10 catheter, and seventy hours after the operation, he was suddenly seized with a severe rigor, which lasted for two hours. He then perspired freely. Countenance extremely anxious-looking. Complained of much pain in his head and back. No tenderness in the perineum or over the abdomen. No albumen or pus in the urine. Nothing to be detected with the stethoscope. Pulse 120; temperature 101·6°.

March 1. Complained of feeling very ill. No recurrence of the rigor. No pain anywhere. Tongue dry and brown. Morning: Pulse 136; temperature 105°. Evening: Pulse 128; temperature 103·6°.

2nd. Evidently much worse. Complained of no pain or tenderness anywhere. Urine scanty, but free from albumen. Nothing to be detected with the stethoscope. Morning: Pulse 136; temperature 104·2°. Evening: Pulse 116; temperature 104°.

3rd. Perspiring freely. Could not be roused sufficiently to understand the questions asked him. Extremities congested. A dark purple petechial-looking rash had appeared over the whole of the body, especially on the chest, where it was very thickly distributed. Tongue dry and brown. Pulse 160; temperature 104·5°.

From this time he gradually passed into a state of coma, and died on the following night at a quarter-past one o'clock, the temperature rising to 107·6° fifteen minutes before death.

Post-mortem Examination (thirty hours after death).—Lungs congested posteriorly; extreme staining of inside of heart and bloodvessels; liver healthy; spleen enlarged, and broke down into a pulp when cut; kidneys presented numerous hemorrhagic spots, varying in size between a pin's head and a small pea, many of these showed signs of softening in their centre; bladder healthy, walls rather hypertrophied. Around the seat of stricture, at the junction of the spongy and membranous portions of the urethra, there was a slight extravasation of blood; the mucous membrane over this part was perfectly smooth—there was no apparent rupture of it, The prostatic veins were considerably obstructed, and one of them was blocked with a firm clot for about an inch and a half.

This, gentlemen, is one case. Let me now briefly read you the notes of the second one, which appeared in our post-mortem room yesterday, and which you have all had an opportunity of observing:—

D. S., aged 46, a muscular Irishman, was admitted into No. 9 ward on December 13. He told us that he had only noticed that his urine came in a small stream during the last two years; that on several occasions he had had small-sized instruments passed, and that once or twice he had shivered after their passage, but this was not the rule.

December 14. A No. 5 sound was attempted to be passed, but only for a short time, as the stricture bled at the slightest touch of the instrument.

15th. Had suffered no inconvenience from the attempted passage of the sound. Ordered ten grains of Dover's powder at night and half an ounce of castor oil the next morning.

16th. Holt's instrument was passed with little difficulty through a stricture in the membranous part of the urethra, which was split with the largest-sized dilator; one or two drops of blood following the operation. Ordered thirty minims of tr. opii and two ounces of brandy to be taken in hot water. In the evening he passed his water freely with little pain.

17th. 4 a.m.: Whilst at the closet was seized with a severe rigor, which lasted for about two hours. 9 a.m.: Temperature 102·2°; pulse 120. Ordered brandy and an opiate draught. 12 noon: Temperature 102°; pulse 130, weak; extremities cold and congested. Ordered five grains of carbonate of ammonia every two hours. 2 p.m.: Pulse weaker. Ordered three ounces of brandy in hot water. 5 p.m. In much the same condition. Brandy to be repeated. 9 p.m.: Temperature 100·2°; pulse 130. Brandy and ammonia to be continued; hot-water bottles to be applied to extremities.

18th. 9 a.m.: Temperature 100°; pulse uncountable. To continue brandy. 2 p.m.: Temperature 101·2°; pulse imperceptible at wrist; extremities mottled purple. Has passed no water during the last twenty-four hours; bladder not distended. Patient perfectly sensible, but restless. Ammonia to be discontinued; half an ounce of brandy every fifteen minutes; to be dry-cupped over loins and to have hot fomentation of infusion of digitalis applied over the abdomen.

From this time his condition changed but little. He passed a small quantity of water at about 6 p.m., and remained sensible to within a short period of his death, which occurred at 3 a.m. on the following morning—sixty-two hours after the operation, and forty-eight after the first rigor.

Post-mortem Examination (ten hours after death).—Moderately firm old pleuritic adhesions on both sides of the chest

lungs healthy, but congested at bases; heart and its valves healthy—a small quantity of clot and fluid blood in each cavity,—no clot in the pulmonary artery; intestines and peritoneum healthy; liver to some extent congested, otherwise perfectly healthy; spleen healthy; kidneys healthy, not even congested. The bladder contained about six ounces of urine; walls slightly hypertrophied; mucous membrane healthy, and not congested, except for a patch about the size of sixpence at the posterior part of the neck of the bladder; the tissues under this were also congested for the depth of about the eighth of an inch. The urethra was slightly congested along its whole tract, and at the juncture of the spongy and membranous portions there was a patch of deep congestion, showing the seat of the stricture. There was a slight extravasation of blood into the tissues around this for the depth of about the eighth of an inch. The mucous membrane over this point, examined under the microscope with a two-inch lens, showed no sign of rupture.

It has seldom been my duty to draw your attention to cases of so painful an interest as the two I have just related. Those of you who saw the first patient will recollect what a fine healthy-looking man he was. You will also remember that in the operation-room I mentioned that it was the slightest case of stricture that I had ever performed forcible dilatation upon. It may be said that in this instance the passage of the catheter on the second day after the operation was the cause of the mischief; but then the subsequent passing of a catheter follows as a necessity upon this method of treatment. In the second case no such question can arise. This man, with all the organs of his body healthy, died in sixty-two hours from the undoubted effects of the operation.

There was a certain amount of resemblance between these two cases. In each the first symptom was a severe rigor, coming on many hours after the operation, and in each there was the same dusky congested appearance of the extremities. Very little either was there to be detected during life, and still less in the post-mortem examinations; for I look upon it as probable that the deep-red condition of the inside of the heart and large vessels in the first case was due to a considerable extent to the effects of decomposition. But the hemorrhagic spots in the kidneys of the first patient were exactly similar in appearance to that which you sometimes see in well-marked pyæmia. Moreover, during life the symptoms and appearance of these patients closely resembled what we see in the worst cases of septicæmia following after other operations or injuries which sometimes, but fortunately very rarely, occur in

the wards of our Infirmary. Therefore I should put down the cause of death in both instances to blood-poisoning.

The practical lesson I wish you to learn is, that in performing any operation upon the urethra you cannot be too careful. When the patient's history points to the stricture being a bad one, or more especially should he habitually have suffered from rigors after the passing or attempted passing of an instrument, let me urge upon you the advisability of insisting upon at least twenty-four hours' complete rest before you attempt to do anything. It has always been my rule to do this; and for two reasons—firstly, you will find it much easier to pass an instrument through a bad stricture if your patient has been kept quiet for some time; secondly, I believe that previous rest often obviates the occurrence of subsequent unpleasant symptoms, for I have more than once found that patients who had always suffered inconvenience from the passage of an instrument would stand considerable manipulation with impunity, previous rest having been insisted upon. At any rate, should mischief result, you will have the satisfaction of knowing that all rational means had been taken to prevent it. Holt's operation is one you have seen me perform moderately frequently, but never without having kept the patient in bed for at least forty-eight hours.

Mr. Holt claims for his operation that cases of stricture treated by this method show less tendency to contract than after any other plan of treatment. My experience relates only to strictures situated in the neighbourhood of the membranous part of the urethra; but it is decidedly in favour of this assertion. At the same time, as a rule, unless a full-sized instrument is afterwards passed with tolerable regularity, you are running your patient to unnecessary risk; for to a certainty will the diseased tissues again contract.

There is one more point of very special interest that these cases help to demonstrate. I mean what actually occurs when you rapidly force the dilator through the stricture. Mr. Holt advances the proposition that the diseased submucous tissue is alone split, and that the mucous membrane is simply dilated. This is a matter of considerable importance, because, with any considerable wound of the mucous membrane of the urethra, there would be a risk of infiltration of urine being the result, though I believe this risk would be slight. We particularly examined both these cases, with the view of obtaining information on this point, and undoubtedly they go towards proving the correctness of Mr. Holt's supposition; but we must bear in mind that during the time these patients lived after the operation a certain amount of rupture might have healed. Still, they certainly seem to prove that, at any rate when the stric-

ture is not a bad one, the injury, if any, that is done to the mucous membrane is of the slightest.

With these two cases before us, it may perhaps suggest itself to the minds of some of you to ask—Why continue to perform the operation? You cannot learn too early that cases of stricture, as a class, are some of the most troublesome and difficult that you will have to deal with, and also that no method of treatment is perfectly free from danger, for death has more than once resulted from the simple passing of a catheter. There are also persons who will not submit to the tedious treatment by gradual dilatation. Specially have I found this amongst the lower classes, with whom time is of importance. These people will remain under treatment till the stricture is partially relieved, and then cease to attend, to come back again in a few months' time worse than ever. But even under these circumstances you would not be justified in performing Holt's operation, if the risk to life from it is considerable, unless the advantages to be derived from it are far more considerable. But what is the increased amount of risk? Sir H. Thompson, in his lectures on "Diseases of the Urinary Organs," lays no stress upon this point, neither is it referred to by many of our best surgical authorities. Moreover, the results published by Mr. Holt are extremely favourable; therefore, we have a right to assume that the cases we have this morning considered are very exceptional. My experience of the advantages of the operation in successful cases is decidedly satisfactory; but unless the patient afterwards submits to the periodical passing of a full-sized instrument, the stricture will again to a certainty contract, and the operation will have been useless.

In treating all ordinary cases of stricture let me advise you, as a rule, to adopt first of all the simplest method at your disposal, and fairly try the plan of gradual dilatation, reserving Holt's operation for the more severe cases, or for those patients who will not submit to lengthened treatment. Remember, also, that there are certain persons whose constitutions resent any interference with their urethra, and who will tell you that they nearly always suffer from a rigor after any attempt to pass a catheter. It will behove you in such cases to be especially careful.—*Medical Times and Gazette, Aug.* 2, 1873, *p.* 121.

66.—ON STRICTURE TREATED BY MR. F. JORDAN'S METHOD PER RECTUM.

By L. Stromeyer Little, Esq., late Surgeon to the London Hospital.

A Portuguese seaman, aged 23, was admitted into the Shanghai General Hospital, suffering from stricture of the urethra.

He stated that for more than a year he had had difficulty in making water, and that for some months he had only been able to do so by drops. He had often had gonorrhœa. He was a healthy muscular man; when examined, a large instrument passed easily about two inches and a half down the urethra and then came to a dead stop. The same occurred with all the various kinds and sizes of elastic and metal bougies. He passed water at the rate of a drachm a minute. He was kept quiet for some days, and then small instruments were tried through a Stromeyer guide catheter, but without success. The urethra, from the point where the stricture commenced down to the bulb, was very hard, but not much enlarged externally. Endoscopic examination afforded only negative results. When the patient had been three weeks in the hospital without any prospect of cure without operation, I determined to open the urethra from the rectum on Mr. Furneaux Jordan's plan, and either pass a bougie forwards from the wound in the rectum, thereby dilating the stricture, or by causing all the urine to pass per rectum for a time, give the strictured urethra rest, and then attempt its dilatation. The patient, after having been made to retain his urine for eight hours, was placed in the lithotomy position under chloroform, and, with the left forefinger in the rectum for a guide, the urethra was opened in front of the prostate to the extent of half an inch at least. An elastic *bougie à boule* was now passed from the wound towards the meatus, and although its point could be felt in the urethra in front of the scrotum it could not be passed further. Other kinds and sizes of bougies were tried with a like result. A large elastic catheter was now passed into the bladder from the rectum, and tied *in situ*, and forty drops of laudanum were given by the mouth.

The patient removed the catheter during the following night, but it was easily replaced in the morning. He did not suffer in any way from the operation. The catheter was retained three days, and on the fourth day, as the patient complained of passing urine by the penis, an attempt was made to pass a bougie through the penis, but without success. Three days later, however, a No. $\frac{1}{2}$ elastic bougie was passed through and left in the stricture. This, after eight hours, was replaced by a larger one. From this date, every morning and evening the bougie was changed for a larger one, until No. 12 was passed. Up to this time the urine flowed entirely per rectum. As the stricture was now sufficiently dilated, a No. 12 catheter was passed every eight hours into the bladder and all the urine drawn off, so as to discourage any escape by the rectum. This treatment was continued for five days, when, on the morning of the fifth, the patient being unwilling

or unable to hold his water until the catheter was passed, voided it naturally.

Examination with the speculum showed that the wound in the rectum was healed. The patient was discharged from the wards, provided with a bougie to keep his stricture dilated. One month later, he presented himself in all respects sound.

Cases of stricture are common among the sailors in this hospital, and are usually treated by gradual dilatation; but as this case presented difficulties which could not be overcome without operation, and as the operation selected is still a novel one, the case is recorded. The operation did not seem to me quite so easy as it is described; but it is, doubtless, a safe way of opening the urethra without a guide, and very much simpler and less severe than by the perineum. Mr. Jordan recommends operating through a speculum, but I had not sufficient confidence in being able—with the speculum—to keep the middle line. In this respect, therefore, I departed from Mr. Jordan's valuable proceeding, discarded the speculum, and trusted to the forefinger as a guide. The subject of this case being a young man, the sphincter ani was very tight, and closed firmly on the finger and knife, hindering their movements. It would do no harm to divide the sphincter muscle, and such a proceeding would certainly make the operation easier. The operation seems suitable to many strictures which are not impermeable, for when the urine has not to pass over the stricture, dilatation can proceed much more rapidly, with less pain, with less inflammation, and, consequently, with less constitutional disturbance.—*Medical Times and Gazette, Sept.* 20, 1873, *p.* 230.

67.—CASE OF RETENTION, FOLLOWED BY SUPPRESSION, OF URINE, LASTING SEVEN DAYS—RECOVERY—OBSERVATIONS.

By Dr. SAMUEL T. KNAGGS, Hon. Surgeon Newcastle Hospital, N.S.W., and Assistant-Surgeon, N.S.W. Naval Forces.

J. G., shoemaker, aged 45 years, living in a suburb near the city of Newcastle. He had been drinking for six or eight weeks. On Sunday, 31st March, 1872, he found himself unable to micturate, but did not suffer any inconvenience until the Tuesday evening following, when at seven p.m. I was hurriedly requested to see him, the case being described as a most urgent one. I found him lying on his back in bed, suffering great distress; pulse 90: respiration hurried; the skin moist and clammy; countenance pale and anxious. He was com-

plaining of great distention in the region of the bladder. He had been vainly endeavouring to urinate, and he described the agony that he suffered as being unbearable. I at once put him into a warm hip bath, which seemed to relieve the most urgent symptoms; while there I made him try to pass water, but the endeavour was fruitless. I then attempted catheterism, first trying most cautiously to insinuate No. 9 gum elastic catheter. It passed as far as the prostate gland without obstruction, and that it stopped. All the usual manœuvres, such as inserting the finger into the rectum, withdrawing the stilette (Hey), depressing the hand, &c., were tried fruitlessly. After a most patient trial, I likewise failed to pass No. 6, 4, and 2. The warm bath, however, seemed to cause an abatement of the most urgent symptoms. At nine p.m., I again saw the patient, in consultation with Dr. Harris. Though somewhat distressed with the feeling of distension about the cystic region, the more urgent symptoms had not returned. The cystic dulness reached almost up to the umbilicus, the prostate gland was found to be very much enlarged, and the cystic tumour was plainly perceptible through the anterior wall of the rectum. Catheterism again failed. We decided upon trying to induce a free action of the bowels; in the event of this failing, the propriety of operative interference could be considered next day. Urgent symptoms, if arising during the night, to be treated by hot stupes and warm baths.

Twenty grains of calomel were at once given in four pills, to be followed hourly by two-ounce doses of black draught until the bowels should be moved.

Fourth day: Wednesday, 3rd April. I visited the patient at day-light, and found that the pills remained down, but every attempt to take any of the draughts was frustrated by incessant vomiting. The patient had been up several times during the night in great pain, unavailingly endeavouring to pass water. After each fruitless effort he found considerable relief by remaining half an hour in the warm bath. Upon examination I found that the cystic tumour had considerably subsided. He complained less of the feeling of distension than he had done the day before. I made a careful examination of the bedding and clothing, but could detect no evidences of his having unconsciously passed water. Symptoms of uræmic poisoning appeared to be setting in, the pupils becoming contracted, an inclination to drowsiness, slight wandering of the mind, and great irritability of the stomach. At ten a.m., according to appointment, I again met Dr. Harris in consultation with reference to the case. By this time the symptoms of uræmia had become more decided. The patient could with difficulty be roused from the comatose state

into which he had fallen. There was incessant vomiting of a greenish watery fluid in copious quantities, having a fœtid urinous odour. The skin was moist and clammy; also exhaling an odour similar to the vomit.

We decided that the time for operation had passed, the abdominal tumour having all but subsided. Catheterism again failed. It being considered very desirable to produce free action of the bowels, croton oil, in two-minim doses, was ordered every hour until the bowels should be moved, the oil to be placed upon the tongue. This caused such an aggravation of the vomiting and thirst that it had to be discontinued after the second dose. In the afternoon the patient was still stupid and comatose, yet could be aroused on his attention being attracted. He said that he had had a beautifully quiet sleep, and now had no pain in the bladder. Catheterism was again tried while he was in a warm bath, but failed. He expressed himself to be greatly relieved every time he got the bath, but in this instance he fainted on getting out of it to go to bed. I now made what I feared would be the last attempt to evacuate the bowels, administering at once a turpentine injection, but his weak state was such that it caused no action of the bowels. I accordingly followed it up with about two pints of warm water, which so distended the bowels that it was almost instantly ejected. Half an hour afterwards he was relieved of a fair solid motion, but he fainted on getting off the stool. This so dejected him that he settled his worldly affairs, having a firm conviction on his mind that he could not possibly outlive the night.

Fifth day: Thursday, 4th April. Patient very weak and low spirited; slept during the night, but wandered greatly in his mind; excessive watery vomiting, profuse clammy perspirations, the excretions having all the same urinous odour. He has not had a morsel of food since last Saturday, and has not passed water since Saturday morning. He has a firm conviction on his mind of approaching death, and says that he will surely die this evening. He is so weak that the least effort produces fainting. There is an inclination to stupor, no actual pain, and the bladder is flaccid.

Sixth day: Friday, 5th April. I tried to induce him to consent to go to the hospital, but his friends objected, preferring him to die amongst them. His appearance much the same as yesterday, but he was much weaker, and hardly able to raise his hand. Another enema was administered, which returned stained greenish black. He said that this relieved him greatly, but the increased vomiting, cold perspirations, and stupor ppeared worse.

Seventh day: Saturday, 6th April. Had a quiet night. Begged to be left quiet on his back, as each movement produced excessive retching. At two p.m. injected per rectum a pint of strong beef tea and two glasses of brandy. He felt almost immediately much stronger. To be continued every four hours.

At seven p.m. he passed a chamberful of water, giving a strong alkaline reaction, after which there was less inclination to vomit. I ordered a draught, containing two grains of oxalate of cerium, one minim of creosote, two minims of dilute hydrocyanic acid, and two drachms of mucilage of gum arabic, with water, to be taken every hour.

The above draught was designed to allay the vomiting and irritability of the stomach. I again saw the patient that night, when he expressed himself as feeling like a "new man." He rapidly improved from this out, and in a week was able to walk about.

Observations.—This case in itself is remarkable, from the fact that the patient, a broken-down debilitated subject, survived seven days without passing a drop of water from his urinary bladder, and ultimately recovered. It possesses a further interest, from the fact that nature came to the rescue of the physician, and indicated a rational procedure in the treatment. The gastro-intestinal tract and the skin took on vicarious action, as was indicated by the copious watery vomitings and profuse perspirations exhaling a peculiar urinous odour. These organs (stomach, intestines, and skin) voluntarily gave their assistance to the kidneys, and performed their functions while their (the kidneys') portals were stopped, and thus relieved the blood of urea and such effete products as would have accumulated in the system, and have literally poisoned the patient.

The mechanism by which the bladder was relieved of its contents on the fourth day can, I think, be fairly explained, as well as illustrated, by our physiological researches in the study of Asiatic cholera. In Asiatic cholera a prominent symptom is the copious rice water evacuations by mouth and rectum; these are thrown out in such vast quantities, and so greatly exceed the ingesta, as thereby to excite surprise as to where the fluid all comes from. Actual experiment has proved that several changes take place in the blood which supplies this fluid. Being deprived successively of its water, its salts, and albumen, then the properties of the corpuscles become changed, and they in turn *rob the tissues through which they permeate of their watery constituents.* There is no obstacle to our assuming that a somewhat analogous change took place in the blood of the patient whose case is under consideration. The gastro-intestinal tract and skin, by energetically taking on vicarious

action, deprived the blood of water, urea, chloride of sodium, as well as other effete matters. The blood having become physically changed in its properties, the corpuscles absorbed the necessary watery constituents from the tissues; the bladderful of fluid in its turn became a reservoir, and, for the time being, recouped the tissues with the necessary moisture by yielding up its contents. Thus can be explained why, though the bladder does not physiologically possess absorbent properties, the absorption of its contents actually took place in this instance.

This case I deem a most instructive one, and if the lesson which it inculcates be carefully studied, and the hints which it suggests be followed out in a spirit of scientific inquiry, it cannot but be productive of much good, and materially modify our treatment of urinary retention and suppression.—*Dublin Jour. of Medical Science, July* 1873, *p.* 27.

68.—ON THE DETECTION AND REMOVAL OF VESICAL CALCULI BY A NEW METHOD.

By WILLIAM DONALD NAPIER, Esq.

[At present stone in the bladder is detected by sound, Mr. Napier, by an ingenious contrivance, proposes to detect it by sight. Not sight of the stone, but of the impression it makes upon the end of his instrument. There is no doubt that stones sometimes escape detection—probably from their surface being friable and soft, and not producing a sound when struck. The instrument proposed, the author says, "will, I hope, render a doubtful result more exceptional than it has ever been."]

Unlike any instrument ever applied to the purpose before, the discovery of the calculus is dependent upon ocular instead of aural proofs,—the outer substance being so peculiarly sensitive and readily impressed by contact with any hard particle, however minute, that evidence of the existence of calcareous matter is obtained, on its withdrawal from the bladder, by the impressions resembling slight scratches that are found on its surface. The black pigment with which the instrument is partially coated is produced by its momentary immersion in a weak solution of nitrate of silver (about one drachm to half a pint of water), with which also the marks it may receive in use must be obliterated. After the application of the solution it should be washed and allowed to dry, and when not in actual use should be enclosed in cotton wool, for the avoidance of any accidental impressions that contact with grit or dust would inevitably cause. As the principle upon which the instrument is constructed renders its success dependent upon the perfection

of its outer surface, it may with advantage be examined through a magnifying glass both immediately before and after insertion, that the smallest blemish or indenture should not pass unnoticed. It is called the Calculus Detector.

It is unnecessary for me to enlarge upon the obstacles that beset the path of the operator who desires to perform with skill and delicacy what is, I think, generally allowed to be one of the severest tests of the surgeon's art, and, when attended with success, one of his most assured triumphs; nor will I do so more than is necessary to my present purpose, which is to point out the more obvious complications that are at times consequent upon the operation of lithotrity, with a view to considering whether their number and gravity might not be diminished, could certain conditions be insured which mechanical art has as yet failed to accomplish.

The performance of lithotrity under favourable circumstances implies the breaking and comminuting of a vesical calculus, so that its fragments may be discharged with the urine; but unless this standard of perfect manipulation is attained in the first instance, difficulties arise immediately on the dispersion in the bladder of fragments too large to find an exit in a natural manner, and the distress that is caused by their presence entails either a premature repetition of the first operation, which is undesirable, or the portions of calculus must be separately grasped with a small lithotrite and drawn away,—a process which causes intense pain to the patient at the time, is constantly followed by protracted suffering consequent on abrasion of the mucous membrane, and not unfrequently results in the production of permanent stricture.

It was when having regard to these various impediments to recovery, whose origin seems to be attributable to the after-treatment rather than to the primary operation, that it suggested itself to me that an instrument might be constructed which by its form and substance should guard the passages from laceration during the withdrawal of the crushed fragment or fragments of stone from the bladder. After eight years of careful consideration of these requirements, I have had an instrument constructed. It is a soft india-rubber tube, terminating at one extremity in a funnel-shaped orifice, greatly resembling in form the ordinary convolvolus flower, and capable of containing a calculus, or portions of calculus, of considerable dimensions. Inserted into the bladder, its form and position render it the receptacle of such foreign substances as would, from the position of the patient and flow of urine, determine towards the natural outlet. This receptacle, enclosing such bodies in the folds of its delicate membrane, and permitting itself to be gently withdrawn, retains them in its

elastic envelope, and protects the urethra from their direct contact.

There is a silver canula, enclosed in which the indiarubber extractor is introduced into the bladder furnished with a cocoa-butter point, which closes its orifice at the same time that it facilitates its ingress by its lubricating properties. The instrument is shown ready for the hand of the operator, who, after inserting it in the same manner that he would an ordinary catheter, ejects the india-rubber tube and cocoa-butter point at the same time that he draws away the canula. The cocoa-butter point dissolves readily in about three minutes.—*Med. Times and Gazette, June* 14, 1873, *p.* 623.

69.—ON THE TREATMENT OF PROSTATIC RETENTION OF URINE.

By JONATHAN HUTCHINSON, Esq., Senior Surgeon to the London Hospital, Surgeon to the Moorfields Ophthalmic Hospital, and to the Hospital for Skin Diseases.

[The object of this paper is to draw attention to some modifications in the instruments for the treatment of prostatic cases.]

The first which I will allude to is the catheter which I now show. It is of metal, of large size, and with a long curve; and differs only from that which since the time of Sir Everard Home has been known as the "prostate catheter" in the position and size of its eye. Instead of there being two eyes, one on each side, there is a single large one placed in front, at a short distance from the end. The object of this is to permit the introduction of a piece of india-rubber tubing through the catheter into the bladder. Latterly I have used for this purpose, not india-rubber tubing, but an india-rubber catheter; having found a very easy method of securing the latter in. Some years ago, having experienced great difficulty in retaining flexible instruments in the bladder, it occurred to me that if I could pass a piece of india-rubber tubing and leave it with six or eight inches coiled up in the cavity of the viscus it would probably be self-retained. We found on trial, in several cases, that such was generally the fact, but not invariably, for sometimes an irritable bladder would succeed in expelling the tube however flexible it might be, and however long might be the portion which we had introduced. We found also that the tube caused more or less of cystic irritation; and thus, although the plan seemed a decided improvement on all others for certain cases, it yet had its drawbacks. It became a problem, on the one hand, to shorten the tube as much as possible, and, on the other, to prevent its accidental expulsion. After trial of several

contrivances, we succeeded in completely accomplishing the latter half of our task by means of the simple apparatus which I now show, and which most of you have, during the last year, repeatedly seen in use in the wards. It consists in principle of a stiletted plug, the stilette being about an inch and a half long, and serving the purpose of preventing the catheter from bending at the meatus. If an india-rubber catheter is made stiff for a very short distance—say an inch or an inch and a half—in the first part of the urethra, and if its end be secured to the penis by tapes, the bladder cannot expel it. We have now never the slightest difficulty in retaining these catheters as long as we like, and it does not matter what size we introduce or how soft and flexible the material is of which they are made. All that you have to do after having introduced the catheter is to put a little metal nozzle with rings into its orifice. This nozzle does not pass down more than three-fourths of an inch, and it is fitted with the stiletted plug which accomplishes the end referred to. The nozzle, having been passed into the meatus, is fastened firmly to the penis by means of tapes and plaster. Take care that it is fixed pretty tightly and with good plaster, and instruct your patient never to take the plug out excepting when he wishes to void urine. If these conditions are fulfilled, you need not be under the slightest anxiety as to the catheter coming out. Your patient may leave his bed, may walk, sit, or go about his ordinary affairs without any inconvenience. I have in several instances had old gentlemen who did this, and who assured me that they were scarcely conscious of the presence of the instrument. You will see that with this stiletted plug the catheter may be shortened as much as you like. The means by which it is retained is in the first inch of the urethra and not within the bladder, and if the catheter be long enough to reach the urine, that is all that is necessary. I have told you that my first idea was to secure retention of the india-rubber tube by letting it coil itself up in the bladder, but that plan has of course been wholly abandoned since the adoption of the stilletted plug. It is right that I should here explain that other surgeons have devised methods of securing india-rubber catheters in place. Thus Mr. Holt has invented an ingenious one with wings, and Sir Henry Thompson has published the description of one which, as regards the prevention of the bending of the tube at the meatus, is the same in principle as that which I recommend. His consists in the introduction within the flexible catheter of a thin German-silver tube four or five inches long. This has to be fitted in place by the maker, and as mine can be changed at a moment's notice and adapted to any catheter, I think I may claim for it considerable practical advantages. As regards

the contrivance of "wings" as means of retention, my experience has been that they are not effectual, whilst they certainly render the introduction of the catheter more difficult.

Having thus explained the means by which the india-rubber is kept in place, I must next return to the question of its introduction. Whilst I was in the habit of using tubing, I was accustomed to pass it down through the catheter which I have shown; and having introduced so much that at least fifteen inches were fairly within the bladder, we could then withdraw the silver instrument, together with the portion of tubing still within it. This done, the tubing was cut through close to the meatus, and the stiletted plug fitted to it. A far simpler way than this, however, is to take a catheter, or piece of tube of the proper length, pass it through the metal instrument or conductor, and then with a sort of ramrod push it down whilst in the act of withdrawing the latter. You may use as ramrod a common flexible gum bougie; but I prefer the metal one which I now show. You will, of course, take care not to push it quite into the bladder—an accident which may be rendered impossible either by using only a short ramrod or by being careful to withdraw the conducting-tube at the same time that you push down the india-rubber one.

I am very sanguine that by the use of the simple means which I have described, we may take a final leave of most of the troubles and distresses for practitioner and patient which have hitherto been so common in connexion with prostatic retention of urine. At any rate, I think we shall be able to manage, with vastly increased comfort both to ourselves and to our patients, all cases in which the patient is seen *early*. If the retention be allowed to persist for several days, and the bladder has been greatly distended, and the urine thrown back on the kidneys, secondary conditions of disease may have been set up which will progress in spite of any subsequent treatment. For all early cases, however, the introduction (and retention if necessary) of an india-rubber catheter is such a simple and safe measure that I confidently predict that it will supersede all others. Here let me state, however, that I do not recommend the retention even of the softest india-rubber unless it is necessary. The necessity for such retention must be measured by the degree of difficulty in its introduction, If difficulty is encountered, if it is necessary to use a silver instrument, and if more or less of pain be caused to the patient at each introduction, ill consequences are sure to follow sooner or later. Although I by no means allege that a piece of india-rubber retained is in all cases innocuous as regards irritation of the urethra, yet I am sure that under circumstances such as those alluded to it is by far the lesser evil. Indeed, I entertain a

great dislike to repeated introductions of silver instruments in prostate cases, even when they are most easily passed. It is true that many patients bear their use three times daily without detriment; but in many others irritation of the bladder, hæmaturia, and cystitis are sooner or later the results. Cases of prostatic retention of urine have, in the past, not unfrequently ended fatally; indeed, it is not many years since one of the foremost members of our own profession died in this metropolis within about ten days of his first symptoms. It is scarcely necessary that I should enlarge upon the value of this plan of treatment in exceptionally difficult cases, and when the patient's residence is at a distance from that of his surgeon. It was, indeed, from my experience of difficult cases at a great distance from home that the importance of perfecting some method for the introduction and retention of flexible instruments became impressed upon my mind. It must have happened to most surgeons of experience to be placed in this position, and to have succeeded in relieving a patient under circumstances which induced the utmost reluctance to withdraw an instrument which had, after so much difficulty, at length been got in. I would strongly recommend that all silver prostate catheters be made on the pattern which I have shown. They are just as useful as others for ordinary purposes, and they supply the means of introducing india-rubber whenever, from the difficulty which has been encountered, it may be thought advisable to retain a tube. The retention of a silver catheter is inconvenient, painful, and even dangerous, yet how often has it happened that the surgeon has been driven to this expedient in the fear that, if he took it out, he might, on the next occasion, fail to introduce it.

And now, gentlemen, I have to advert to another point in the management of this class of cases of yet greater importance than the contrivances which I have been describing. Although, probably, it is well known to some members of the profession, I am sure that it is not a matter of general knowledge, that *in almost all cases of prostatic retention a flexible india-rubber catheter without any stilette can be passed into the bladder*. The india-rubber which I use is very soft, and so flexible that, as you see, you may tie knots in it. Yet the cases are very exceptional in which it cannot be got in, and it will in many cases pass readily when the use of a stiff instrument is difficult. The explanation of this is that it can follow the windings of the distorted urethra. In prostatic cases the urethra is not narrowed, but is merely lengthened and pouched and bent by the projecting lobes of the enlarged gland. The india-rubber is just stiff enough to permit of its being pushed down the urethra when there is no impediment, and it is

flexible enough to allow of its following any windings, and mounting over any projections that it may encounter. In using it without a stilette there is of course no scope for either skill or dexterity. All that you have to do is to oil it well and then push it in inch by inch. If it stops, withdraw it a little and then try again. In most cases the patient can do it just as well as the surgeon. It causes no pain, and scarcely ever produces bleeding. Its introduction is indeed so simple and easy that it may be repeated two or three times in the day without any risk, and thus you may avoid the necessity of retaining it. I have not the least hesitation in asserting that the time-honoured silver catheter ought at once to be superseded by the india-rubber one, and that no one ought ever to think of trying metal or even gum-elastic unless he has failed with one of those which I now recommend.* It will also in the future, I hope, only be in very exceptional cases that the means which I have recommended in the first part of my lecture will be found necessary. I have myself of late scarcely ever failed to get the india-rubber in, and in a few in which I have been unable to do so the conducting silver catheter has served an excellent purpose. Nor is it only in prostatic cases that you will find india-rubber catheters useful. They are very valuable for exploration of the urethra in cases of suspected stricture, since they cause the patient less pain than any other kind. They will prove useful, also, for retention in cases of ruptured urethra, and perhaps in cases of paraplegic incontinence.

Now and then, even in cases of stricture, in which the canal is irregular, and the stricture perhaps in part due to spasm, you will find unexpectedly that a small-sized india-rubber instrument can be got in. I have succeeded with them in one or two cases of this kind greatly to my astonishment, even after some difficulty had been encountered with metal.—*Lancet, June* 7, 1873, *p.* 797.

AFFECTIONS OF THE EYE.

70.—ON A NEW METHOD OF EXTRACTING CATARACT BY PERIPHERAL SECTION OF THE IRIS, WITHOUT INVADING THE PUPIL.

By Dr. CHARLES BELL TAYLOR, Surgeon to the Nottingham and Midland Eye Infirmary.

[However much a previous iridectomy may facilitate the extraction of a cataractous lens, and may obviate the tendency to

* These catheters are not vulcanised, but are perfectly smooth on the surface. They are much more pliable than those which have until lately been in use. They are made by Mr. Warner, of Tottenham. They may be procured of Messrs. Krohne & Sesemann, Duke Street, Manchester Square, and probably of most instrument makers.

inflammatory action subsequently, the cosmetic defect resulting from the sacrifice of a portion of iris is so considerable as to counterbalance its advantages.]

Before the adoption of Von Graëfe's, Mooren's, my own, and other modern methods of operating, prolapse of the iris was always considered the bugbear of extraction. We have got rid of this danger by the sacrifice of a segment of iris, and the question that presented itself to my mind was, whether the same end might not be attained at less cost by merely removing a minute portion of the periphery of that membrane, so as to obviate the risk of prolapse during the few days following the operation, and yet, in the end, maintain the pupil in all its integrity, and secure the chief advantage of the old flap method without its principal attendant danger. This end I have attained by excising a small piece of the outer edge of the iris, and through the aperture thus obtained, separating it with very fine scissors from its marginal attachments to an extent corresponding to the size of the wound required for the easy exit of the lens.

The sphincter of the pupil is thus not stretched by the passage of the cataract which glides from its posterior surface; the iris is not drawn out of the wound during the excision of the minute portion it is necessary to remove. The wound is so small that there is no bleeding from the iris to obscure the future steps of the operation, and the portion of iris corresponding to the wound so frequently the initial seat of fatal inflammatory mischief escapes the bruising which Von Graëfe's researches have shown was so frequently the cause of disaster in Dariell's method.

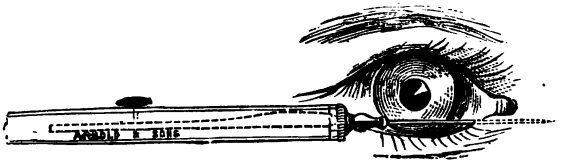
I find also, as a matter of experience, that a slight wrinkling, to be seen only when sought for, is frequently the sole change to be observed in the margin of the iris after the recovery of the patient, while, as a result of the procedure, the pupil is maintained intact, central, and moveable. I have in some cases endeavoured to facilitate the first part of the operation by the preliminary application of the calabar bean, and the adoption of a minute section in the corneo-sclerotic junction at the summit of the globe, as also by occasionally performing the minute irodectomy six weeks before the extraction. These precautions, however, may generally be dispensed with, and the section completed at once, either by the small knife which I commonly use for extraction, or Von Graëfe's. The capsule may be lacerated either through the slit in the iris or the natural pupil.

Since devising this method I have operated in the manner described upon thirty-five cases. In two of these all chance of vision has been lost; three had iritis with closed pupil, and in three others, owing to prolapse of the vitreous, the marginal

pupil was larger than I could have wished it. In the majority, however, the result was very brilliant, and quite equal to the most successful flap extractions. The operation is somewhat tedious, but may be easily enough performed with care—a fact which, through the kindness of Mr. Bader, the distinguished ophthalmic surgeon of Guy's, I have been enabled to demonstrate on more than one occasion to the students of that hospital.—*Medical Press and Circular*, *April* 2, 1873, *p.* 285.

71.—INSTRUMENT FOR SLITTING THE PUNCTUM LACRYMALE AND CANALICULUS.

Mr. GREENSLADE has lately invented a little instrument which will certainly prove very useful in ophthalmic surgery. It is manufactured by Messrs. Arnold and Sons. It consists of, as will be seen in the adjoining woodcut, a canaliculus director, and knife, combined in one handle, thus permitting the opera-



tion of opening the duct to be performed with one hand, whilst with the other the surgeon depresses the eyelid. The knife is so constructed as to be hidden in the handle, and with one finger can readily be slid along the director when in the punctum.

By this simple little contrivance, it will be seen that all danger is obviated of wounding other structures than those required to be divided in opening the duct—an accident hitherto liable to occur with an unruly patient.—*British Med. Journal*, *June* 14, 1873, *p.* 673.

72.—ON STRYCHNIA IN AMAUROSIS.

By Dr. JULIAN J. CHISHOLM, Clinical Professor of Ophthalmic and Aural Surgery in the University of Maryland, U.S.

M. J. H., of Kentucky, aged forty, has been under medical treatment for four years, for progressive optic-nerve atrophy, both eyes being equally defective. He had been in the habit of using tobacco and whiskey freely, and, from the great nervous irritability exhibited after an over-indulgence in tobacco, his physician had very properly considered this the

cause of his defective vision. He was advised by him to abstain from the use of both tobacco and whiskey, which he acquiesced in. Medical treatment was then instituted, and modified from time to time, but with no good results, vision remaining very cloudy. At the end of the four years of treatment and abstinence he had lost, rather than gained, ground. At this time he applied for treatment at the Baltimore Eye and Ear Institute. I found his strength of vision, with either eye, No. 16 of Jaeger's test-type at one foot. This type corresponds in size with the heavy type on the title-pages of books. An ophthalmoscopic examination of the vitreous chamber gave clear media, an apparently healthy retina, with retinal vessels of good size, but the optic nerve-disks were white.

As this was one of a class of diseases, for the correction of which experience had taught me the value of strychnia, I determined to bring this patient under its stimulating influence without delay, notwithstanding his statement that he had already tried the remedy, and had found no benefit from its use. For safety, I commenced with the dose that is usually prescribed by physicians, viz., one-thirtieth of a grain of the sulphate of strychnia; but unlike the majority of the profession, who seldom exceed this amount, and therefore get no good results from its administration, I only used the one-thirtieth of a grain, as a minimum dose, to guard against idiosyncrasies. As this quantity was well borne, it was increased by small additions from day to day, until I reached the largest amount which the system would tolerate. In referring to my note-book, I find the increase in this particular case as follows: $\frac{1}{30}$ gr., $\frac{1}{24}$ gr., $\frac{1}{20}$ gr., $\frac{1}{15}$ gr., $\frac{1}{10}$ gr., $\frac{1}{8}$ gr., $\frac{1}{7}$ gr., $\frac{1}{6}$ gr., and $\frac{1}{5}$ gr., the interval of time required by the system to tolerate the largest dose, from the commencement of the treatment, covering a space of two weeks. These doses were given three times a day until one-tenth of a grain was reached. With the larger dose the night-dose was badly borne, causing too much excitement of the nerve-centres, accompanied with very uncomfortable stiffening of the leg and spine muscles. It was found that the large doses could be taken with no discomfort in the morning, but not in the evening, on account of the slow elimination of the drug, so that by active absorption the night-dose was added to that portion of the morning-doses still in the circulation. This night-dose was, on this account, always made the smaller dose, viz.: when one-fifth of a grain could be taken after breakfast and dinner without much discomfort, one-tenth of a grain was all that the system would tolerate after supper. Under this course of treatment, keeping the patient constantly excited by increasing doses of strychnia, a marked improvement

in vision showed itself even within the two weeks. The patient then returned to his distant home, with instructions to keep up the remedy in full doses. An appreciable stimulation was daily experienced from one-half of a grain of strychnia a day, when continued for even five weeks, and the improvement in vision, although slow, was steady. By this time the system had so fully tolerated the remedy that the usual stimulation evidenced by the toning up of the muscles was no longer experienced by the one-half of a grain, and the improvement in vision seemed to be arrested. For three weeks the one-half of a grain was continued daily, but no progress detected. The dose was now increased to seventeen-thirtieths of a grain per day, when the stimulating effects were again renewed, and within twenty-four hours there was an appreciable improvement in the sight. This patient has, at this time, been taking these large doses of strychnia for eleven weeks. In general health he is stronger and more vigorous than for many years. Now his sight has so much improved that he is beginning to read ordinary print, and will soon be ready to resume active business. This case, one taken from a very large number of cases, is reported in order to show why it is that strychnia, the chief of nerve-stimuli, so often fails to sustain its well-deserved reputation, because administered in doses too small to be of any service. When given judiciously, in doses as large as the system will tolerate, constantly increasing the dose with the tolerance, so as to induce continually an appreciative excitement, good results may always be expected from strychnia in cases of deficiency of nerve-power. The size of the dose must be regulated by the stimnlating effect and must be increased as this stimulation diminishes under the remedy. So far I have had no occasion to exceed one-fourth of a grain of the sulphate of strychnia at a dose, although a case may yet show itself in which this large quantity may fail to produce the necessary nerve-stimulation, under which circumstances I would not hesitate to increase the amount at each dose. After many careful experiments, I have found no appreciable difference between the hypodermic and mouth administration, and now give the medicine in the form of sugar-coated granules, to conceal the bitter taste.—*New York Med. Journal, Feb.* 1873, *p.* 187.

73.—ON BELLADONNA IN THE TREATMENT OF IRITIS.

By JABEZ HOGG, Esq., Surgeon to the Royal Westminster Ophthalmic Hospital.

In my opinion it is in the early or acute stage of the disease, and when certain well-known symptoms are present—as scle-

rotic and ciliary injection, extreme tenderness of the eyeball, circumorbital pain, lacrymation with intolerance of light, and dimness of sight, slight change in the colour of iris, with an increased vascularity which impedes its motion, so that the pupil can be seen to contract and dilate slowly on the withdrawal or admission of light—that a free application of belladonna or atropine is of the utmost value and importance. The late Dr. Mackenzie thought highly of this remedy in the treatment of iritis. "Belladonna," he writes, "ought to be employed in every case, and in all stages of the disease." The late eminent Von Graëfe, speaking of the treatment of this affection, says, "Although I can easily understand that some practitioners are not successful in the mydriatic treatment of acute iritis, because they proceed without discrimination, still, I cannot understand how they can yet consider it irrational. In inflammation of a muscular tissue, what indication is more urgent than to keep it quiescent, at the same time relaxing its most irritable and most threatened parts, in the present case, the pupillary margin? Is there a method more likely to induce re-absorption of an exudation than to deprive it of its source of nourishment? and does not the diminution of intra-ocular pressure facilitate the choroidal circulation, and afford an especial safeguard against the complication with choroiditis so much to be dreaded?"

This is good practical reasoning, for beside the danger to the iris by the formation of adhesions between the posterior capsule of the lens, posterior synechia, there is the danger of the inflammation rapidly proceeding to affect the choroid and retina. It is at the pupillary border of the iris the disease seems to commence, whence it quickly spreads to the deeper seated structures, and the danger to guard against in all attacks is an inflammation of the adhesive kind, which, if not met by active remedies, produces obliteration of the pupil by the rapid effusion of coagulable lymph. It is quite possible that the inflammatory symptoms may, after having run a certain course, abate, and wear themselves out. In such instances the attack may be attended with only a small amount of pain and inconvenience; this has led to a belief in the minds of some practitioners that they may discard such remedies as mercury, iodide of potassium, &c., in the treatment of iritis, but if such a course be pursued, contraction of the pupil—*atresia-iridis*—will most assuredly be followed by total loss of sight.

Every care must be taken to preserve the iris intact, and prevent damage to its muscular fibres from the inflammatory effusion of lymph, which is at times thrown out in the course of a few hours, and without much pain; this can only be done by treating the case actively from the first; and by making a free

use of mydriatics. We can scarcely expect to place the iris at rest, and out of harm's way without belladonna, or the instillation of a solution of atropine; and this gives time for other remedies to act. Should the eye prove at all intolerant of atropine, and which it sometimes is, either from the loaded state of the vessels of the iris, or from a slight conjunctivitis, a local abstraction of blood will be needed before its use is persisted in. This will be best affected by the application of a leech—just below the lachrymal tubercle—and far enough away from the angle of the eye to avoid chemosis. When the leech falls off the bleeding should be arrested by pressure, and in a short time it will be seen that the eye tolerates the solution of atropine or belladonna, and the pupil expands under its influence. As during the night the disease often makes great progress, and as whilst asleep there is a natural closure of the pupil, which must in some measure favour permanent contraction, it is as important that the belladonna should be freely applied to the brow at bed-time, as that the instillation of atropine should be persisted in morning and mid-day.

As the extract of the pharmacopœia is generally too hard for use as an external application, I order it to be mixed with a few drops of glycerine, and applied on oiled silk—this prevents it from drying up; when dry it no longer produces mydriasis. Solutions of atropine will occasionally set up a good deal of conjunctival irritation, and cannot be proceeded with on this account; but this often depends upon the character of the solution employed—it may be too strong (2 to 4 grs. to ℥j. of water is of sufficient strength), or the solution may be made with *spirit*, as ordered in the liquor-atropia of the "British Pharmacopœia." This is clearly a mistake, as the spirit invariably produces inflammation, or irritation and pain. The chief difficulty appears to be from want of care in the preparation of the commercial salt, and which almost always contains a certain quantity of *free acid*. The alkaloid must be *perfectly neutral*, and dissolved in distilled water. Dr. Mackenzie pointed out an occasional effect of belladonna, which he thought might afford grounds for objecting to its use in the acute stage of iritis, namely, its action on the proper substance of the iris, causing it to contract, and at the same time leaving the *uvea* attached to the capsule of the lens. This is a rare and very occasional effect, and one I have never seen, and therefore venture to think in no way detracts from the great value of the drug; it is one less likely to occur when the case is treated in the ordinary way, that is with mercury or iodide of potash, as well as atropine.

The advantage of the mydriatic treatment is not confined to one stage of the disease; it is equally useful in general iritis;

constitutional or specific, as well as in the secondary form of the disease, even that affecting strumous children; and when it occurs as an extension of the inflammation from some other texture—keratitis. In connection also with injuries of the cornea and iris; or in a purely sympathetic character, as when the opposite eye has been injured.—*Medical Press and Circular, April* 30, 1873, *p.* 373.

DISEASES OF THE SKIN, ETC.

74.—ON ACNE AND ITS TREATMENT.

By Dr. TILBURY FOX.

Acne, when fully developed, might be defined, and is usually simply defined, as an inflammatory state of the sebaceous glands, and their follicles, and perhaps the perifollicular tissue, due as a rule to irritation induced by the plugging up of the duct by inspissated secretion, which may perhaps have undergone some chemical change whereby it has acquired an acrid quality. But this description does not satisfy me; for the inflammatory condition in acne is modified as to its extent, its persistence, and its character by various conditions, such as hereditary peculiarity; by constitutional or diathetic conditions, as struma; by derangements of internal organs, as dyspepsia; by altered states of the blood-current, as in gouty persons; and by local influences acting directly upon the seat of the acne, as external irritants. It is necessary, therefore, to regard acne as composed of a primary and essential condition, the plugged and inflamed gland and duct; and a number of secondary items, which influence and modify this inflammatory state, and which must be looked at from the physician's point of view.

First, as regards the general principles of treatment. These consist, as regard preventive measures, in keeping up the general health, and especially a vigorous action of the skin functions, so that the follicles may be kept emptied of their contents. Good friction of the healthy skin, and thorough ablution with a non-irritating soap, are good to this end. But I need not say more on the point than this, that those causes which lead to hyperæmia of the face should not be allowed to operate.

The treatment of acne itself, as a whole, consists in preventing acne punctata from passing on to acne vulgaris, by getting rid of obstruction in the glands, by checking the hyperæmic condition; acne indurata, in diminishing hyperæmia and promoting the absorption of inflammatory products; and acne

rosacea, in checking hyperæmia, and destroying the new growth of connective tissue. Now, it will be observed how much stress I lay upon checking hyperæmia; and this is to be effected not in one, but in various ways, since the causes of its presence are various. Hyperæmia may be controlled by internal remedies, by local remedies, or by removing conditions that by reflex action gave rise to its continuance. But its disappearance may always be helped out by excluding the air from the face as much as possible. In the chronic stage of the disease, revulsives and irritants may be used; but I am sure harm is always done if the disease be acute, or, in its early stage, if the hyperæmia be severe, by remedies other than those that soothe. Why should we treat an hyperæmic or inflamed skin-gland differently from that of any other inflamed gland or part?

Next, it is necessary to prevent the operation of the several causes of intensification of hyperæmia to which I have referred, and especially dyspepsia, uterine disorder, constipation, and a loaded state of the system. Having done this, we may tone up; and remove, by special means, the consequence of the hyperæmia, viz., the induration by stimulants, absorbents, astringents, and caustics, if need be; and counteract any strumous tendency by cod-liver oil and the like, and so prevent or diminish the implication of the perifollicular connective tissue or the pus formation, if excessive.

Now for the application of these principles to the treatment of the several classes of cases that come to the practitioner for treatment.

For practical purposes, cases of acne that most commonly come to the physician for treatment are divisible into four classes.

In the first may be placed the young boy or girl of lymphatic temperament, come, perhaps, of a phthisical or strumous stock, and with languid circulation, pallid countenance, more or less anæmic conjunctivæ, and belonging to a family amongst the members of whom acne is common. Such a patient may suffer from no dyspepsia, but the tongue is not clean, and the bowels are irregular. The indications as regards the general health are those of debility and semi-anæmia; the acne, in fact, being more or less hereditary, perhaps its development is favoured by the debility connected with the lymphatic temperament. The acne attacks the face and back. If the strumous tendency of the patient is marked, the acne spots have a large, more or less livid, base; whilst the suppuration in their centre is marked, and the spots leave behind well-marked cicatrices.

Now, in such cases as these, a general tonic regimen is needed, with good diet, good air, moderate exercise, regular hours, frequent bathing, and the avoidance of sedentary habits

and occupation. There is usually malassimilation, and, as I think, especially of fat, which indeed is avoided in the food. The medicinal treatment required for this malassimilation consists in the use of the mineral acids with bitters, pepsin, with iron if the patient be pallid, and, finally, iodides and cod-liver oil, and the fatty matters in the way of fish, eggs, milk, butter, cream. Ioduret of iron, or arsenic if preferred, is to be commended. But it is likewise necessary to exhibit simple aperients to get the bowels to act regularly. The cod-liver oil should be pushed in proportion as the evidences of struma are marked. As regards local treatment, this depends upon the degree to which the acne is developed. Supposing there be very little hyperæmia about the acne spots, and they take the form rather of comedones than those of acne simplex, it may be advisable to wash the part very freely with a mild soap and water, using gentle friction subsequently, together with an astringent or a mild sulphur wash or ointment.

For my own part, I avoid doing this—which is supposed to loosen and get rid of the sebaceous plugs in the follicles—if there be dyspepsia present, or the face is in any degree liable to become flushed under stimulation of any kind.

The German plan is to soap almost everybody for acne. Well, the English and Germans, in my experience, are not the same people. I much prefer, where there is hyperæmia, hot-water bathing, or bathing with a mild alkaline wash of carbonate of soda, and the use of the mildest astringents. I scarcely ever use anything but a calamine lotion, ℥ss. to ℥vj. fluid, with gr. i. bichloride of mercury, and ʒ ij. of oxide of zinc.

The truth is, that as soon as the general condition improves, the acne mends, and, locally, what is needed is the prevention of any undue inflammatory action. Some cases are very obstinate, especially where the avocations and status of the patient render proper care of him or herself, or the obtaining of proper nourishment, an impossibility. When the disease becomes chronic and indolent, the vessels of the affected spots lose their tone and need stimulating. Whilst the inflammatory induration can only be got rid of by stimulants, I prefer a weak tarry or mercurial application to anything else—ʒ ss.-ʒ i. of the huile de cade to ℥i. of lard, or 5 or 6 grains of the nitro-oxide and ammonio-chloride of mercury to ℥i. of lard. Some cases do well with a mild alum and sulphate of zinc lotion. What I want to bring out prominently for notice, however, is the necessity of avoiding such remedies in the early stages of acne in the patients to whom I am referring, and until the general health has been improved, and the hyperæmia has lost some of its activity.

Another, and, indeed, the commonest kind of case, is that which occurs in those who are the subjects of dyspepsia, and who are debilitated, who suffer from constipation, and are, it may be, anæmic. Any number of these cases may be observed in hospital practice amongst domestic servants, seamstresses, dressmakers, clerks, porters, and, in private practice, amongst people following similar avocations, and in those of the middle and upper classes who live unhealthily. These dyspeptic acne patients, if I may so term them, often get little exercise, live and work in a vitiated atmosphere, in crowded, ill-ventilated workrooms; their diet is often scanty, and their meals are hurriedly taken; and then, at the time that the circulation in the face becomes more than usually active, glandular derangement occurs, excited, perhaps, by exposure to great variations of temperature, and aggravated by the dyspepsia and the debility. Such patients tell you they always flush and feel distended after meals, when the face burns and is redder than usual; and this excitation of the vessels of the face is kept up by the dyspepsia. The urine is loaded with lithates, and the bowels are sluggish. These are the easiest cases of acne to treat. They all get well. They need aperients, antacids (I prefer a gentian and soda, prussic acid, and ammonia mixture, a dose being taken an hour before each meal, with bismuth for a time, if there be pyrosis), together with moderate exercise in the open air; the avoidance of beer, sugar, pastry, seasoned dishes; and, when the tongue cleans, the exhibition of iron and quinine and arsenic combined. The arsenic, I believe, acts as a nerve tonic, and not as a specific against acne. Locally, I never use anything but the calamine lotion before mentioned, followed by a weak sulphate of zinc and alum lotion, three or four grains of each to the ounce. The use of these local remedies should be conjoined with that of hot fomentation, night and morning, and the avoidance of all soaps.

Most practitioners commence at once with the internal use of arsenic and iron in these cases. I feel confident that the removal of the dyspepsia is the proper thing to be first effected, and I can speak from a rare success in the management of these cases of acne. In some of these cases, there is more or less disorder of the catamenial function—amenorrhœa may be present; but the uterine functions are restored to their natural state when the general health improves under the influence of tonics.

But these same cases of acne in dyspeptic subjects are seen, not only amongst the lower and the more indigent of the middle classes, but amongst the upper and well-to-do classes. The dyspeptic and the general lethargic condition which exists in these patients is the result of the inactive and luxurious lives

which they lead, the late hours which they keep, and the rich diet of which they constantly partake at all kinds of evening hours, till the bodily functions become torpid, the general system enervated, and the stomach and excretory organs oppressed, dyspepsia and debility being two of the main results. These cases are to be treated as other dyspeptic cases, only the more care is needed in the prescription of diet. Plain and wholesome diet should alone be allowed, wine and condiments, pastry, and sugar be proscribed; whilst hygienic measures, cold-bathing, horse exercise, change of air, and like measures, calculated to invigorate the body, provided the face is not exposed to external irritants too much, are to be commended. The form of acne is usually that described as *acne vulgaris*—it may be *A. indurata* if the disease has lasted a long time.

But I have two more classes of acne cases to consider. The next kind of case which I may deal with, is that which is observed in patients in good health, and in the condition which the older writers would have described as plethoric. These patients are not particularly young. There may be a certain amount of dyspepsia in their case, very often none; but there is oftentimes a loaded state of system. The bowels are inactive; the kidneys do not excrete freely and fully; the pulse is full; there may be some headache. The face is not only studded over with acne spots, but is more or less generally congested. These patients are oftentimes what may be not at all inappropriately, in their cases, styled, gross feeders. They eat plentifully, and oftentimes take a full quantity of beer or other stimulant. Some exhibit apparently a gouty tendency. The acne spots are well marked, numerous, and large in size; they possess indurated bases, and are easily inflamed. The back and face and the forehead are the parts attacked in these cases. Now, the main thing to be done in such circumstances is to "cut off the supplies" for a while, and to give aperients with a sufficiently free hand, together with mild tonics, and, in gouty subjects, colchium in addition. After a while, tonics alone, with or without arsenic, may be given. The combination of aperients with tonics in these cases was the secret of Mr. Startin's success, I believe. It stands to reason, that, if the emunctory organs have not been doing proper work for some time in these acne subjects who "make blood fast," the blood-current will be unduly charged with effete products, which depress the powers, and so induce debility indirectly; and, moreover, set up, or at least intensify, local congestions—such as acne of the face. Such a prescription as the following may be written:—Sulphate of magnesia, ʒ ss.; aromatic sulphuric acid, ℥ xx.; sulphate of iron, gr. iij.; quinine, gr. i.; colchicum

wine, ℥x.; syrup of ginger, ʒi.; and water, ʒj., twice or thrice a day, with an aperient pill if needed. It soon reduces the facial hyperæmia, the more so if the diet be unstimulating; in practice, stimulants should be interdicted, and the patient be directed to take a course of Vals water, which keeps the kidneys active, and prevents any undue acidity of stomach. By-and-by, after the face is less heated, and the acne spots less inflamed, the colchicum may be omitted, and some preparation of arsenic substituted for it; but a gentle aperient action is to be kept up all the while the patient is under treatment. Now, as in the two other kinds of cases before described, the local treatment must always be soothing at the outset. But, as the general state improves, more active remedies may be used in these plethoric subjects, and I think sulphur is especially useful in them. I use generally a weak sulphur ointment, gr. x. to ʒi. of adeps, to begin with, and prescribe, in addition, a simple calamine lotion as a protection to the face; but if the sulphur does not succeed in improving acne indurata, I then prefer touching the acne spots with acid nitrate of mercury, so as to lead to their resolution; or if this be objected to, or be inconvenient, I apply mercurial plaster at night.

In some cases, the acne spots are very large and inflamed, and, indeed, the whole gland, in its deepest part and its surrounding connective tissue, is actively involved in the inflammatory swelling. I think this condition is one most benefited by the free use of aperients with iron, and also, if the patient be strumous, of cod-liver oil. But a course of Kreuznach or other waters seems, in some cases very efficacious, if the acne is chronic and holds to the patient with any special pertinacity.

The last kind of case to which I shall refer is that which occurs in middle-aged men and women, and takes the form of acne rosacea. Some writers have disputed much whether this is in reality an acne at all. It is not of much consequence whether we class acne rosacea with acne or chronic inflammations of the skin. The disease is a composite affair. It is a chronic inflammation of the face made up of acne spots, periglandular inflammation, erythema, and new growths of connective tissue formed independently of the glands. The first stage consists in congestion of the face, and more or less dilatation of certain capillaries. Particular points of the papillary layer become hyperæmic; and certain of the glands are similarly affected, so that acne spots are produced, an excessive amount of sebum is often secreted, and the skin feels greasy (seborrhœa). The general surface of the skin of the face is more or less congested. In the next stage, as the consequence of the continued inflammation, the connective tissue

around and about the glands hypertrophies—that is to say, the acne spots become indurated and hard; and the independent non-glandular papules become more marked. The colour of the redness is bright red; the vessels become varicose, and ramble freely over the surface of the diseased parts; suppuration is not very marked, but the integument generally is thickened as a consequence of the general congestion. Now, this disease is rarely seen in the young. It occurs in women of middle age who suffer from uterine troubles, and attacks the nose and the cheeks by preference. The disease is aggravated by trouble, by stimulating food, by exposure, by dyspepsia, and by alcoholic drinks.

It is difficult to manage. Some of the cases combine in them the special features of the "dyspeptic" and "plethoric" cases, but then there is no "physiological gland activity" element in the case. The truth is, that the face is generally congested, and the glands are involved as part of the face. The pustulating points indicate the seat of the affected glands, and the minute vessels running up to the indurated base are the best indication of the seat of the newly-formed tissue—the hyperplasia. The skin generally is, as I have said, more or less hypervascular.

I suppose these cases are seen most commonly in women of nervous temperament who have suffered from some time from dysmenorrhœa or even menorrhagia, and who have experienced much mental excitement or anxiety; or in men who have indulged freely in alcoholics or lived badly. The women are often affected by atonic dyspepsia as well. In the management of these cases, what is needed is the rectification, in the first place, of the concomitant condition of ill health, leaving the acne to take care of itself for the while, so far as internal remedies directed to cure it are concerned. The diet in all cases must be unstimulating; alcoholics are to be avoided; when the tongue becomes clean and all evidences of dyspepsia are disappearing, and the uterine functions are restored to their proper state, then the acne itself may be treated. In cases in which there is dysmenorrhœa, with anæmia, and in which the face gets worse before the periods, the combined use of bromide of potassium and perchloride of iron with simple aperients, and if the flushing of the face is marked after meals, a course of Vals water has succeeded in doing much good in my hands. Supposing that there is much induration about the individual spots, if the patient is in fair health and there be no dyspepsia, a mild mercurial course may be prescribed with much success. I give 1-20th or 1-24th of a grain of a bicyanide of mercury with quinine, twice a day for three weeks or so, to be followed by 5 or 8 drops of Donovan's

solution twice a day for some little time; cod-liver oil being likewise exhibited at the same time if desirable. Supposing the skin of the face generally to be thickened, I think Donovan's solution the best remedy to begin and continue with. If the amount of blood in the face varies under slight influences, then I prefer saline aperients with iron and quinine. In those cases in which the patient seems to be suffering from what we know as "nervous debility," then I have recourse to arsenic without hesitation, and push its exhibition in connexion with quinine and allied tonics or nux vomica. In the case of the man who drinks, and has semi-pickled his gastric mucous membrane, the difficulties are great. The best course to adopt is first to send him to drink the waters at Aix, Harrogate, or Kreuznach as a preparatory treatment, and then to exhibit arsenic and iron with aperients.

But there is one thing more to be said about these cases of acne rosacea. It is the necessity of ascertaining if there be any gouty tendency or syphilitic taint about the patient. If the patient be gouty, so much the more reason for a course of Kreuznach waters, and if syphilitic, for a mercurial course; although, as I have said before, a mercurial course may be beneficial in hastening the removal of the inflammatory indurations. In very chronic acne indurata, and still more so in syphilitic cases, the forehead and temple regions are specially involved.

Locally, the sooner the acne spots are destroyed by acids the better. But then my argument will be the same here as in regard to other phases of acne. So long as any concomitant condition which leads *per se* to an intensification of the hyperæmia of the face exists, so long will it be necessary to withhold stimulation and to use soothing remedies in order to keep hyperæmia—I mean of course when that is active—in check; and I hold it to be doubly necessary to observe this rule in dealing with *acne rosacea*, since in that disease there is a large amount of hyperæmia, not merely of the glands and periglandular tissue, but the derma generally. In the early and active stage, then, of acne rosacea, I am content to use hot water and poppy bathing, with inunction of oil or cold cream, followed by the application of warm lead and opiate lotion, the face being diligently protected with a powdery layer deposited from a calamine lotion. Then stimulate with hypochloride of sulphur ointment, or apply a zinc and alum lotion, and touch the several spots with acid nitrate of mercury as they spring up or are obstinate in disappearing. This plan will best rid patients of their trouble.

But I hasten to add, that though I have set forth in order the general treatment of acne, yet I do not forget, of course,

that acne cases come to us for treatment at all stages, and under varied circumstances, and it is the duty of the physician to adapt his treatment thereto.

I have said nothing as yet about the diagnosis of acne, and will content myself with one practical remark, that in middle-aged men and women, but the latter especially, there is a condition of face which is universally regarded as acne (*A. rosacea*), but which has nothing to do with acne. The patient comes to the physician stating that she has had the rash on her face for perhaps several years. It gets a little better at one time, but only to become worse again subsequently. It developed in the form of little red pimples scattered over the face, especially about the cheek and sides of the nose. If a careful inspection of the spots be made, they will appear like little fleshy lumps, varying in size from pins' heads to a quarter of a split pea; they are dull red, and they look fleshy—they are, in fact, little new growths, neoplasms—and have no central aperture indicative of the opening of the follicle,—in fact, are clearly formed not at the follicle, but in the papillary layer. On inquiring into the antecedents of the patient, a past history of syphilis may be made out: the patient has had sore throat, which ran a chronic course; she miscarried several times, or had still-born children; and has had a sore tongue. Perhaps there are evidences of past ulceration about the fauces—in fact, indistinct evidence of a syphilitic history may be occasionally obtained. I only briefly refer to these cases, and will only add, that they are cured by antisyphilitic remedies and tonics, whilst other means fail.

In conclusion, I may be permitted to say, that although I have not perhaps brought forward anything new, yet I hope to have put the facts connected with the development and the course of acne in a truer clinical light, and to have indicated with tolerable clearness what are the exact principles upon which the treatment of the disease should be conducted.

I have tried to show, *firstly*, that acne must be regarded as being modified in degree and aspect by a variety of accidental conditions, that in treating acne it is necessary to deal with these conditions, and to negative their influence before, or at the same time that, the acne itself is treated; *secondly*, that in the early stages of acne the only admissible or successful treatment is one that soothes the hyperæmic surface. Stimulants and revulsives must only be had recourse to in the chronic or indolent stages, and after the influences of agencies that aggravate or that keep up the hyperæmia have been checked.—*Edinburgh Medical Journal, May* 1873, *p.* 1005.

75.—CASES OF RINGWORM TREATED BY OLEATE OF MERCURY.

By Dr. LEONARD CANE, Queen Square, W.C., London.

In introducing the use of oleate of mercury, in a clinical lecture published in the Lancet on May 25th, 1872, Mr. Marshall mentions its applicability to certain skin diseases, and the record of the following cases of ordinary ringworm (tinea circinata) treated by oleate of mercury may be serviceable.

Case 1.—Here there was a well-defined, slightly-raised circular patch of tinea circinata on the side of the neck, about the size of a two-shilling piece. The spot was rapidly extending itself, and its edge was marked by a circle of small vesicles, situated on a slightly inflamed base. The centre presented a "branny" appearance, owing to its being covered with fine brownish scales. These scales when treated with liquor potassæ were found under the microscope to contain numerous spores and threads of fungus.

On June 3rd, a few drops (about twenty) of the oleate of mercury (10 per cent. strength) were *gently* rubbed over the spot with a piece of lint. The oleate was applied beyond the diseased patch.

On the third day the patient was again seen. The sight of the patch was marked by a flat, circular, reddened spot of the same dimensions as the original. There were no vesicles, and no traces of any fungi could be found. The spot appeared to be completely cured, but, as a matter of precaution, it was deemed advisable to reapply the remedy. It was therefore dabbed over the surface, about five drops being used.

Since then there has been no reappearance of the disease. In this case the oleate caused no inconvenience whatever. There was no staining of the skin, no pain after application, and the cure was rapid.

Case 2.—This was a much more severe case. The disease had been treated by various remedies—namely, by glacial and ordinary acetic acid, by iodine, &c.,—and although these had stopped its progress for a time, it had invariably broken out afresh. When first seen the disease was extending almost round the whole of the back of the neck, from the angle of the jaw on one side to that on the other. It was also spreading very fast up into the hair, and for a distance of more than an inch the hair on the back of the head was invaded by the disease. There were also isolated patches on the chin, below the angle of the mouth, and on the upper lip. There were

several scars where glacial acetic acid, and I believe also strong nitric acid, had been applied, and in one place there was a troublesome sore produced by the strong acid, which was kept up by the rubbing of the collar. The disease had existed for several months, and a second case had occurred in a house where the patient had been staying whilst the patches were on his neck. The newer spots were well marked and highly characteristic, and afforded abundance of sporules, &c. The site of the older ones was reddened by inflammation, and discoloured by the application of the various remedies mentioned.

On June 16th, after cutting off as closely as possible all the short hairs, so as to remove as much of the fungus as it was possible, I applied the oleate of mercury (ten per cent.) over the whole of the affected skin, gently rubbing it in with a piece of lint. About half a drachm was used altogether. The isolated patches on the face were similarly treated. Care was also taken to rub the hair above the diseased part with the oleate. The patient was then ordered to wear a loose collar and not to wash his neck during the day. Some slight irritation followed the application, but this was trivial, and not for a moment to be compared with the pain produced by strong acids, &c. There was no staining of the skin, and, as the spots were in a prominent position on the face, this was no slight advantage.

On the 18th patient was again seen. There had been no fresh appearance of the disease, and the old spots had not extended themselves. There was still considerable redness over the old scars, and a few minute pustules produced by *rubbing* in the oleate, but there were none of the small vesicles which had been so evident on the former occasion. The oleate was reapplied over the places to ensure success.

On the tenth day after the first application the disease appeared to be completely cured. There were several pale red patches showing where it had been, whilst the spots on the face and chin had quite disappeared. The hair had begun to grow again. and showed no trace of the spores. The sore produced by the acetic acid before he came under my notice had scabbed over, and was now nearly well. A third application was made, as a precaution, where the hair had been affected.

When seen a short time afterwards, the skin appeared perfectly well, and there has since been no reappearance of the disease.

The advantages which oleate of mercury seems to possess over other remedies are:—

1. It is a *certain remedy* if carefully applied.

2. It *produces no staining* or injury of the skin. In cases where the disease appears on the face, it is of great importance to avoid any disfigurement or staining.

3. It is *painless* in its application. This is not the case with the ordinary strong parasiticides, most of which produce vesication, &c.

4. It *readily penetrates* into the sebaceous glands, hair follicles, and even into the hairs themselves, the mercury being in a state of solution in an oily medium, and it is therefore much more likely to destroy the fungus than the spirituous or aqueous solutions of mercury, &c. This penetrating power of the oleate may be increased by adding a small quantity of ether (one part to eight) to it.

In very sensitive skins the irritation sometimes produced by it may be avoided by using a weaker solution (five per cent.), and by applying it with a camel's-hair brush. In slight cases this method is all that is necessary, but where the fungus has invaded the hair it is advisable to rub in the oleate gently.—*Lancet, Aug.* 16, 1873, *p.* 227.

76.—ON THE TREATMENT OF ULCERS OF THE LEG.

By Dr. J. GORDON BLACK, Surgeon to the Hospital for Sick Children, Newcastle-on-Tyne.

The perusal of the valuable reports which have appeared in the Journal on the treatment of ulcers, at the various London hospitals, induces me to offer a few remarks, in the belief that good will accrue from the further ventilation of the subject.

It seems pretty generally admitted, that the treatment of ulcers of the leg, in the out-patient room, is unsatisfactory and disheartening. Whilst some instance the intemperate habits, the poorly fed and over-worked condition of the patients, to account for this want of success, I am more inclined to blame a wide-spread belief in the profession, as expressed by Mr. Lawson, of the Middlesex Hospital, that "for the effectual treatment of all ulcers of the leg, absolute rest of the limb is the first element." Having for some time past been in the habit of curing cases of this kind without requiring the patient to neglect his ordinary duties for a single day, I certainly cannot hold such a belief. If it be possible to cure a large ulcer of the leg without rest, and in quite as short a time, to say the least, as would be required to heal the same by recourse to the horizontal position, what becomes of the theory that absolute rest is necessary?

Moreover, during the time that the patient remains in bed, the circulation through the limb is rendered more efficient, and healing of the ulcer ensues; but no sooner are ordinary duties

resumed, than the old conditions recur, bringing back with them the inevitable ulcer. A cure, therefore, under the absolute rest system, can scarcely be alleged, because it is not permanent; whereas, if an ulcer be healed without rest, it is clear that a cure has been effected, provided similar therapeutic conditions are maintained. This latter provision cannot, of course be observed under the former plan of management.

Knowing, as I do, the easy practicability of healing ulcers without rest, I cannot think it proper to admit such ailments to the wards of a hospital. To do so, seems wasting the funds of the institution, no less than the time and labour of the sufferer. At the same time, it is not creditable to surgery that such patients should be neglected, or given to understand that their weary and loathsome malady is incurable.

The plan which I adopt is practically the same as that recommended by Baynton, nearly eighty years ago, but with the important modification of using it antiseptically. Baynton's strapping has long been recognised as valuable and effective, and is described by Mr. Erichsen under the head of "Indolent Ulcer." Such management, however, taxed too severely the time and patience of the surgeon, for its successful practice. Unless the plasters were very frequently removed (Mr. Erichsen says every forty-eight hours, at least), the pent-up discharge became very offensive, causing the dressings to be disagreeable, both to doctor and patient.

In order to avoid these disadvantages, I now warm the plasters by passing them through hot water, to which a little solution of carbolic acid has been added. The sore having been washed clean by the patient, is then saturated with weak solution of carbolic acid, and the straps, first treated as described, applied. The pieces of plaster (stout emplastrum saponis), should be two inches broad, and long enough to overlap four inches, after passing completely round the limb. They should be applied after the manner of a "Scot's dressing," from about three inches below the lowest diseased surface, to about the same distance above the highest. In their adjustment I think it most important to use no compression, but simply to lay them down evenly, so as to fit the limb accurately, and leave no creases in the plaster. Should pain be produced, the strap has been improperly applied, and must be at once removed. The bandaging of the limb, lightly and carefully, from the toes to the knee, finishes the dressing, which latter need not occupy more than ten minutes altogether. The patient may be told to return at the end of a week, when, on removal, the plasters will show only a slight moisture, instead of the profuse and offensive discharge seen when no antiseptic is used.

The advantages of the above plan of treatment are briefly

these. It is cleanly; it saves the time and labour of the surgeon, for the dressings need rarely be changed oftener than once a week, and occupy only a few minutes. And, finally, whilst the healing process is conducted with a minimum of pain and discomfort to the patient, he is in no way restricted from pursuing his ordinary occupations.

In cases of irritable ulcer, with much pain, Baynton recommended the sufferer to remove the bandage occasionally, and pour cold water upon the strapping for a few minutes, afterwards drying lightly with a soft towel, and reapplying the bandage. The plan is an excellent one, and usually very grateful to the patient's feelings.

Instead of employing carbolic acid, another good antiseptic may be used, namely, sulphurous acid. This is easily applied by playing upon the ulcer and surrounding diseased skin with a Dewar's spray-apparatus. The plasters may then be adjusted, after passing them through hot water, simply. A little smarting ensues, which, however, soon passes off. The effect of the sulphurous acid, in checking discharge and mal-odour, is quite as good as that of the carbolic, whilst its application is perhaps less troublesome and disagreeable. The sulphurous acid is especially suitable to ulcers of moderate size.

Baynton's strapping, especially when used antiseptically, may be employed for nearly all kinds of ulcers. The surface of a weak, indolent, or inflamed ulcer, speedily assumes a healthy appearance, without the preliminary use of astringent, soothing, or other lotions, being necessary. The most irritable sore may be strapped if care, and no compression, be used. Occasionally, however, it may be found advantageous to substitute linen or calico for the plaster straps.

For varicose ulcers, no treatment could be better. The horny edges of the "callous" variety quickly disappear, without recourse to such a dangerous excitant as blistering, which may easily set up unmanageable inflammation in the old or infirm. In eight or ten dressings, even very extensive ulcers may be healed by strapping, so that the cases must be few where skin grafting is really needed.

The administration of medicines internally is unnecessary. In most cases opium may be given to relieve pain, but the healing process goes on steadily, without such assistance.

I observe that Mr. Nourse, of Brighton, has used strapping and bandaging with great success. The plan, I feel sure, only requires more extensive employment to be better appreciated. For the frequent failure of the ordinary treatment by lotions, &c., patients are often blamed, because they do not strictly obey the instructions given. This neglect is, however, due rather to the well-known feebleness of such remedies, than to any lack of

pains or inconvenience on the part of the sufferers. On the other hand, the interest which the latter manifest in carrying out directions under the treatment by strapping, is by no means the least recommendation of this method. — *British Medical Journal, May* 31, 1873, *p.* 608.

77.—ON SOME POINTS IN THE SURGICAL AND MEDICAL TREATMENT OF CHRONIC ULCERS OF THE LEG.

By Dr. JOHN KENT SPENDER, Surgeon to the Mineral Water Hospital, Bath.

More than forty years ago my father devised an ointment containing a very large quantity of prepared chalk, in order to form an artificial crust over an ulcer. Let us ask ourselves what occurs when nature alone accomplishes the cure, and which may be sometimes observed in the case of injured animals. Sir James Paget tells the story in his usual lucid way: —"Sometimes . . . the blood shed from a wound coagulates and dries on it, and, remaining as a scab, permits healing under it; or, if this do not happen, a similarly effective scab may be formed by the serous fluid or lymph by which the surface of an exposed wound usually becomes glazed; or, more rarely, the pus of a granulating wound may scab over, and sound healing take place beneath it." We see, then, that there are three agents which may be concerned in forming a protective scab over a recent wound—blood, lymph, pus. A scab constructed of either of these materials shields the part from outward injury; and, thus covered and guarded, a new skin is formed, and the crust drops off. When the cure is left to nature, it is only a superficial ulcer which readily scabs; for if the ulcer be deep the quantity of matter is too great to be speedily evaporated into a crust. Therefore our object should be to assist the natural process by the addition of some harmless substance which will thicken the discharge and produce an incrustation; and this is very efficiently done by covering the ulcer or wound with the ointment described above. The chalk must be in a much greater proportion than that which enters into any ointment in the Pharmacopœia, consisting of about three pounds of chalk to two pounds of lard. And the best way of preparing this application is, not by rubbing the chalk down with the lard, but, having previously reduced the chalk to a very fine powder, melt the lard in any convenient vessel over a slow fire, and then add gradually the chalk to the liquefied lard. Stir and thoroughly mix the two until nearly cold, and the ointment is then ready for use.

Now, it will be found that several capital advantages accrue from the use of this ointment:—1. It generally produces much ease and comfort. 2. When the lard becomes melted by the heat of the part, the chalk is disengaged, and a portion of it combines with the secretion from the ulcer. This secretion is often so acrid as to excoriate the neighbouring skin; but, when united with the chalk, it is converted into a neutral innocuous compound. 3. A crust is formed out of this compound; first on the surrounding skin, then on the margins, and finally on the surface of the ulcer. 4. Little disturbance of the applications is necessary. At first the quantity of the discharge may require the frequent renewal of the dressings; but this will be less and less needed as time goes on, and our object should be to maintain the mechanical integrity of the chalky incrustation.

The ointment should be spread smoothly and pretty thickly on the soft side of good French lint, and the ulcer and a fair circumference of the adjacent skin should be covered with it.

This preliminary operation being completed, a bandage should be put round the limb with surgical neatness and care. I almost always use bandages made of "domette flannel;" this material is thin and yielding, and yet almost any degree of compression can be exercised with it. In œdematous swellings generally the flannel is soft to the skin, and accommodates itself to the greater or less distension of the limb. The calorific properties of flannel, too, are useful, as in all structures in which the capillary circulation is obstructed the vital heat is reduced. The bandage should be at least six yards long if required for an ordinary male adult; the breadth should be under two inches. Every portion of the limb, from the toes to the knees, should be equally and evenly compressed. Indeed it may be said that good bandaging is of such absolute importance in the treatment of varicose ulcers of the leg that without it everything else will be comparatively ineffectual. This being so, the rapidity and completeness of the cure will depend very much on the manner in which it is employed. Without practice it is not easy to place a bandage properly on the leg; and probably this difficulty is the chief reason why preference is often given to adhesive plaster, as this sticks and remains wherever it is put. The blistering and excoriation often produced by strapping, and the time consumed in the application of it, are sufficient reasons for acquiring skill in the art of bandaging—an art whose comforts and advantages are appreciated by a patient commensurately with the dexterity with which it is practised by the surgeon.

In late years I have sometimes employed bandages made of a very thin and somewhat elastic calico. They are suitable to

persons who have an irritable skin, and they are generally useful in summer time.

Now I recommend the chalk ointment above described, not because it has any empirical virtues, real or supposed. No one pretends that any therapeutic miracle or mystery is worked by it. The plea advanced on its behalf is simply this—that it admirably helps the natural healing process, and it helps nature by imitating her. Accidental circumstances may in any case prevent nature from being so easily imitated and so smoothly put right; but the experiment is always worth a fair trial. When unforseen difficulties present themselves, the first impulse is to "try" all sorts of unusual remedies, in the hope that by pure chance the right remedy may be found out. A philosopher's healing stone will perhaps be discovered in this way some day; but at present it is more safe to be guided by principles of common reason, and we shall thereby sometimes reap a success when we least expect it. The literature of this subject is very desponding, but we shall clear the road a little if we point out some rules and landmarks which will now and then save us from going quite wrong.

Next, let our inquiry be, What ought to be done when we have to treat an ulcer which not only does not heal, but which obstinately wears a foul and unpromising aspect? In the first place, then, the use of a nitrate of silver lotion (one scruple to one ounce of distilled water) stimulates a healing surface, and often prevents it from degenerating into superficial gangrene. Every time the leg is dressed, the ulcer and the circumjacent skin should be painted over with this lotion; it acts as a true sedative, even though for a little while it may cause a trivial smarting. Suppose, again, that the sore is always covered with a thin greenish slough, what are our resources for this state of things? Steep a pledget of "chloralum wool" in equal parts of water and the solution of chlorinated soda (liquor sodæ chloratæ, Ph. B.); fill up the cavity of the ulcer with the saturated wool lightly teazed out, and cover it with a chalk-ointment dressing. No disinfectant is more potent than chlorine, and in this little-used preparation of our national Pharmacopœia we have a means of applying it locally and effectively. It is, in my judgment, very much superior to carbolic acid in its cleansing power; and the latter has such a disgusting smell as almost to need some disguise to make it tolerable. Even the weakest solutions of carbolic acid are apt to irritate and even inflame a delicate skin, and this fault can never be alleged against chlorine. I can speak in the highest terms of "tarred oakum" as an application to superficial sloughing sores in the coarse, thick integument of wayfaring people, and I have used it much in dispensary practice. The

stuff called "tenax" is good for more delicate folk, but it is hardly enough impregnated with the tarry principle.

"Senile gangrene of the integument" is the name given to a subtle but formidable disease, considered by some surgeons to be a variety of senile scrofula. A dirty, buff-coloured slough creeps along, until large tracts of skin may become involved; and very severe pain is a frequent concomitant. The necessity of arresting (if it be possible) this slow molecular death ought to be perfectly clear; otherwise it may lead to remote but fatal septicæmia. Now, what are we do do? Are we to cover each patch (say on the foot and leg) with a linseed poultice, with the idea of getting rid of the dead skin and finding a clean granulating surface underneath? I have seen this plan tried, and with the natural result of increasing the sloughing area. Heat and moisture are precisely the agencies which hasten decomposition and encourage any approach to it; and the sloughs, instead of being loosened and shed, are much more likely to extend. Moreover, I have found that the pain is even increased by the persistent application of poultices of any kind. Out of a number of cases of "senile gangrene" which have come under my care during the last few years, I relate one which illustrates pretty clearly what seems best to be done.

At the end of last November I was asked to visit a lady, 67 years old, who had a black gangrenous patch, four inches long by an average of an inch and a half broad, on the lower and back part of the left leg. The general health was bad (there was an attack of nearly fatal syncope on the second day of my attendance), the local pain was great, and the disease was slowly extending. There had been no medical treatment, and only some simple cerate had been applied to the sloughy surface. The whole of this was brushed with a solution of nitrate of silver (strength specified above); it was then dressed with lint on which was spread chalk ointment, mixed with an equal part of ointment of benzoated zinc; and, finally, I put on a domette bandage (with moderate pressure) from the toes up to the knee. In about ten days (the dressing having been renewed daily) a profuse "laudable" secretion loosened the slough at its edges; within ten days more it came entirely off, leaving a healthy granulating surface, which healed pleasantly and gradually before the end of January. The general health was sustained with tonic medicines and an abundant supply of protein food.

Poultices may be fine and useful things, but only an utterly wrong estimate of their capacities would lead the surgeon to

apply one to a sore which is unhealthy or mortifying from intrinsic sources of decay.

The second topic of my paper relates to the non-necessity (and even uselessness) of absolute rest of the ulcered limb, provided that the mechanical element of the treatment be sound and sure. In this matter the principles of surgery seem to have retrograded, if we compare a recent summary of the "Treatment of Ulcers" in some London hospitals with what we may read in the "Practical Observations on the Cure of Wounds and Ulcers on the Legs, without Rest," written and published by Mr. Thomas Whately, in 1816. Mr. Whately tries to obey Pott's immortal maxim, that "it is the business of good surgery to assist nature;" and, in the third chapter of his excellent book, after a minute relation of the way in which an ulcerated leg should be dressed and bandaged, he says that "from the first application of the bandage we may freely allow the patient to follow the most laborious employment, and to walk or stand the whole day. I can with great truth say that I have scarcely ever found any impediment to the healing of these wounds by such a constant motion, provided that they were dressed and bandaged in the manner just described."

The great dogma of rest has been enforced with much eloquence; but I venture to assert that the therapeutic pendulum has swung too far that way, and we are in positive danger now of a caricature of the whole thing. Firm and equal compression with a bandage answers every object to be gained by the recumbent posture; and the help which this may be theoretically supposed to afford by no means counterbalances the acknowledged evils dependent upon the loss of constitutional exercise. Continuous rest, on account of a small local lesion, implies (assuming the body to be otherwise healthy) a large capital of bone and muscle lying fallow, and even deteriorating from want of employment. Further, it is with many persons a matter of serious inconvenience and cost to abandon all active pursuits for weeks or months together; nor is the subject of less moment in the economy of hospitals, in which we often find beds occupied by cases of ulcer of the leg to the exclusion of other more urgent and certainly more interesting forms of disease.

An immense array of facts in my experience have practically settled this question, and authorised me to state that the treatment of *ordinary* cases of ulcer of the leg does not require any rest of the limb at all. Any casual attack of inflammation must of course be met with suitable measures, and these will include rest and an elevated position; otherwise the moderate use of a limb, when properly bandaged, is not only no bar to

the cure of a varicose, or any other variety of ulcer, but is an actual help to it. To lay up a part of the body in idleness admits of the healing of an ulcer by a degree of vital action, which is unable to maintain the permanence of that healing when the part is restored to its natural use. It is only fair to add that Dr. Underwood and Mr. Chapman fully recognised the value of exercise as a curative stimulus; and more modern writers (as Mr. Hunt and Mr. Houghton) have endorsed the same view.

It is a point of good practice to bandage a leg enlarged and varicose, although no ulcer may have yet appeared. We note the signals of coming danger, and, if our science be so often at fault in the attempt to remove an evil, we ought to discover in this the stronger inducement to ward off its approach.

The *medical* management of the various forms of ulcer ought not to be disparaged. The clue of syphilis should be sedulously followed up to its source, and treated with precision accordingly. To Mr. Maunder we are indebted for a useful diagnostic observation. He says that all those sores which are situated *above* the middle of the leg are syphilitic in their origin, and are mostly multiple also; while the varicose ulcers are found *below* the middle of the leg, and are usually solitary. Gout and lead poisoning may aggravate and exasperate the most innocent ulcers; and chronic alcoholism is the most potent influence of all in causing universal degradation of tissue. So far as medical treatment is concerned, a large number of non-inflamed chronic ulcers are curable, or made much better, by a course of citrate of iron and iodide of potassium, with now and then an aloetic purgative.

This is not the place to discuss the large question of skin-grafting as a surgical remedy for ulcers. I tried it in a few cases during my long tenure of office at the Eastern Dispensary here, with doubtful results; but in private practice I have never had occasion to resort to it, so great has been my success with the plan of treatment described in this paper.—*Lancet, May* 3, 1873, *p.* 623.

78.—TREATMENT OF CHILBLAINS.

Mr. Fergus (Canada Medical Journal) recommends sulphurous acid in this affection. It should be applied with a camel-hair brush, or by means of a spray-producer. One application o this usually effects a cure. The acid should be used pure. A good wash for hands or feet affected with chilblains is sulphurous acid, 3 parts; glycerine, 1 part; and water 1 part. The acid will be found particularly useful in the irritating, tormenting stage of chilblains.—*Medical Press and Circular, Aug.* 27, 1873, *p*, 201.

79.—ON SUTURES, AND THE TREATMENT OF INCISED WOUNDS.

By Dr. J. C. OGILVIE WILL, Aberdeen.

Sutures are undoubtedly the best means we possess for maintaining the edges of cut parts in apposition. Unfortunately their employment is attended by certain disadvantages: such as the irritation, giving rise to suppuration, when silk is the substance of which they are composed; the danger, during their removal, of tearing apart loosely united surfaces; and the pain experienced by the patient when they are constituted of metallic bodies. Another grave objection to the use sutures is, the unsightly marks—consequences of suppuration—which remain at the points where they had been inserted. These cicatrices are of course chiefly objectionable in parts exposed to view, as in wounds of the face, &c. The object of the present paper is to draw attention to sutures formed of a substance which can be employed without fear of the foregoing undesirable effects presenting themselves. This substance is catgut, which Professor Lister brought into notice some years ago for this purpose, but which he has since discarded, though he still retains it for ligatures. Regarding its use for the latter purpose I cannot speak personally, as I have always found acupressure answer every purpose; but with regard to the former I can speak most emphatically, for during the last two years in every wound where I have had reason to expect early union—and it is only in such cases that I advocate its use—I have invariably employed catgut sutures, and their employment has been attended by the happiest results. In proof, I may cite a few cases.

1. A child came against a pane of glass in a window, broke it, and in so doing almost severed the fleshy part of his nose from the cartilages. The surfaces of the wound, having been washed and exposed to the air for a few minutes, were brought into apposition, three catgut sutures were inserted, and cold-water dressing applied. In three days the sutures were removed, and a day or two after hardly a trace of the injury remained. The sutures did not give rise to the slightest irritation.

2. An injury causing a somewhat similar but much more severe lesion, the cartilage of the septum being fairly divided, occurred to an old woman. In this case the edges of the wound were very jagged. The same treatment was adopted, with like results.

3. Case of excision of the mamma. Eight sutures; one vessel acupressed; pin removed seven hours after; immediate

union. The sutures, not giving rise to the slightest inflammation, were allowed to remain till the thirteenth day after the operation, when they (or rather what remained of them) were removed. Shortly after the points where the sutures had been inserted could not be discerned.

4. Another very striking case was that of a young child who was brought to me with the point of his middle finger hanging merely by a thread, like a piece of skin, the whole of the other tissues, including the bone, having been divided. After carefully cleansing the wounded surfaces, and allowing them to become glazed, I brought them into accurate apposition, and inserted two catgut sutures, remarking at the time to the boy's mother, that I had little hope of a favourable result, and that I would probably have to remove part of the finger, but that I had given him a chance. Fortunately I was not obliged to resort to such a proceeding, as the parts healed kindly, and on the sixth day I removed the sutures. On that day only one small point remained unhealed.

5. Amputation of the third finger, with removal of part of the metacarpal bone, in the case of a boy. Three sutures; wound healed without the slightest suppuration. The sutures were removed on the seventh day, when the child was sent home perfectly well.

6. Same operation as last, on a man. Three sutures; one vessel acupressed; pin removed four hours after. Œdema and slight suppuration, notwithstanding which the sutures were allowed to remain; and on the twelfth day the wound had entirely healed.

This case showed that catgut may be employed with advantage in a different class of cases from that in which I have advocated its use, as, unlike wire, which will cut its way out, and silk, which will resist, catgut will stretch to a certain extent, I might quote many other cases of wounds in other parts, but must content myself with the above-mentioned.

If surgeons will only give catgut sutures a fair trial, at the same time not disregarding other essential precautions to which I shall presently refer, I feel assured that they will have every reason to feel satisfied with the results following their use. The other points to be attended to, in the treatment of incised wounds, are that there be no foreign bodies in the wound—such as blood, dirt, or "dead pieces of meat," as Sir James Simpson quaintly termed ligatured vessels,—and that its surfaces be exposed to the air till they have taken on a glazed appearance. This last indication is not, I am aware, in accordance with the views entertained by many surgeons at the present moment; but experience here has proved that union takes place more readily when air has been freely admitted than when wounds have been

hurriedly closed. It is probable that if there are spores in the air of other places, Aberdeen has its due share of them, but they do not exert the baneful influence ascribed to them in some other cities; and the fact that union by the first intention has been obtained here in so many instances of amputation, excision, &c., after they have been exposed to free currents of air, for periods varying in duration from ten to thirty minutes, seems to me to be a proof that the germ-theory of putrefaction in wounds is hardly a tenable one. The only explanation that could possibly be given by the supporters of that theory would be that the air in this city is pure and free from germs; but to explain the why and wherefore of this exemption would be a still more difficult matter. Even if this could be done, it would not be sufficient; for in Edinburgh, where germs are supposed to exist in their highest degree of development, I saw more than one case of wounds after excision of tumours unite *without one drop of pus*,—after Sir James Simpson, not content with the ordinary access of air, used a *bellows* to ensure every part of the raw surfaces being, so to speak, flooded with it. Further, regarding dressings, I find the less the better; when any are employed, a simple strip of wet lint suffices. I have not thought it necessary to enter upon the necessity of obtaining accurate coaptation of the edges of cut parts, as this point is so universally recognised as a *sine quâ non* in the treatment of incised wounds that even mention of it seems superfluous. The treatment I have indicated is applicable to all incised wounds; excepting that I would only counsel the use of catgut sutures in those of the slighter order, or in those where, from from the nature of the case, there is reason to expect union by the first intention.

I may add that, as I have received several applications from medical men requesting me to obtain for them a supply of catgut, what I have employed has been prepared by Messrs. Young and Son, North Bridge, Edinburgh.—*Lancet, June* 28, 1873, *p.* 907.

80.—SPINA BIFIDA CURED BY INJECTION.

By Dr. J. R. WATT, Consulting Surgeon to the Fever Hospital, Ayr.

The following case of spina bifida occurred in my practice some time ago, and was considered an excellent opportunity for testing Dr. James Morton's method of treatment by the injection of a solution containing ten grains of iodine and thirty grains of iodide of potassium in an ounce of glycerine: this, with Dr. Morton's cases, make three treated in this manner, all of which have been successful.

On June 28th, 1872, Mrs. M. R., an anæmic woman, was delivered of her third child, a small and weak boy of full time. In the position of the third, fourth, and fifth lumbar spines, was a sessile semi-transparent flesh-coloured fluctuating tumour, as large as an ordinary-sized hen's egg, and so sensitive that simple contact of the finger brought on fits of crying, during which the investing membrane became very tense. On firm pressure with the finger, a vacant space, bounded at the sides by bony ridges about half an inch apart, was found in the usual position of the vertebral spines; and, when viewed by the transmitted light in a darkened apartment, cords were seen passing near the surface. A pad of cotton-wool was placed over it under a firm binder, and the child was daily seen for a week. It moaned almost constantly, slept little, and took the breast badly. Its legs dangled, and no voluntary attempt was made to move them—slight movement of the toes being the only reflex action from tickling its soles, and the sphincters seemed very weak.

On August 2nd, the tumour was much larger, measuring eight inches in circumference. The most prominent part was ulcerated superficially; the lower border overhung the sacrum. Two drachms of clear fluid were removed by Wood's syringe (which was used throughout), care being taken to avoid the nerve-cords. The cotton pad and binder were replaced; and the child was ordered to have half a teaspoonful of spirits in water every three hours.

On August 3rd, the tumour was one-half less; from the needle-aperture drops of clear fluid still oozed, and from the soaked condition of the pad and binder a constant escape must have gone on. The child looked much exhausted, but had had no convulsive symptoms.

August 11th. The tumour was as large as before interference. From its upper edge a second quantity of two drachms was removed. The child was kept with the face down on the nurse's knee, with the back uncovered; and the aperture closed in a short time.

August 17th. A third quantity of two drachms was removed, and half a drachm of Dr. Morton's solution was injected. The child cried immediately. The spirits and water was continued.

August 18th. The child had slept very little, and appeared very weak. The tumour was unchanged in size, and along the left border a faint blush was present, which faded in about four days.

August 26th. The tumour was slightly decreased; the membranes were lax, and the fluctuation-wave less distant.

September 13th. The tumour was nearly level with the back, and was replaced by a large half-dried resistent scab.

The child was firmer; the legs never moved together, but when the soles are tickled one or other leg was drawn up a short distance. The sphincters were more under control.

February 21st, 1873. A cake of firm condensed tissue occupies the site of the tumour. The child was much grown, plump, and thriving.

It may be remarked that, even although the tumour and the cavity of the spinal membranes communicated, as shown by the nerve-cords in view, and the greatly depressed fontanelle after the first withdrawal of fluid, yet no irritation spread along the cord from the injected solution, and the result must be considered successful.—*British Medical Journal, April* 26, 1873, *p.* 462.

81.—THE TREATMENT OF ALOPŒCIA.

By HENRY FINCH, Esq., Colchester.

A gentleman, who had lost nearly all his hair after a very severe attack of fever, consulted a French physician of great reputed success as a hair restorer. The prescription was a drachm of the tincture of phosphorus to one ounce of castor oil; the bare spot to be rubbed with this mixture three times weekly for half an hour each time, after the skin of the head had been thoroughly cleansed with warm water without soap. This treatment was faithfully carried out for about six months; the hair soon began to grow, and in a year from the time of first following the doctor's advice his head was as thoroughly covered as ever, the new crop of hair being about two shades darker than the old.

On several occasions I have used the same means, slightly supplemented, and always with some success. I commence with having the few thin, straggling, and colourless hairs on the bared spot cut as short as possible with sharp scissors; the skin is then thoroughly cleansed from scruff by washings in warm water. I then direct a skull-cap of oil-silk or gutta-percha, fitting well round just above the ears, to be made, and in this every night to be applied a warm linseed-poultice, well moistened with castor oil instead of water. In some folk the linseed causes an ugly-looking superficial irritation of the skin, with thick yellow discharge. I then use bread. Such a case I had quite recently in which the linseed caused a straggling, superficial sore, with swelling and discharge, taking some fourteen days to heal, and leaving for some time a disagreeable dark mark. In the morning, when the poultice has come off, the head is washed, and then well rubbed for a quarter of an hour or twenty minutes with homœopathic tincture of phosphorus, half a drachm to an ounce of castor oil, or about

half per cent. of the phosphorus. Any hairs that appear are carefully cut with scissors. This treatment must be continued three or four times weekly for some months, the head never being allowed to get dry, but from time to time moistened with some kind of oily preparation for the hair. It is in no way inconvenient, a good thick night-cap of stocking material being worn to avoid soiling the pillow-slips, and keep the poultice *in situ*. Of course if the scalp is polished and smooth as a billiard-ball, and you can detect no delicate thin hairs with a strong magnifying-glass, the probability is that the hair follicles have ceased to be productive, and I believe no treatment can renew their fruitfulness; also in cases where baldness is hereditary little success is to be hoped for. The poultice continuously applied, with the resulting warmth and moisture, I fancy to be the strong point of the treatment; the oil only three times or twice weekly. And if anyone doubts the efficacy of this treatment, let it be tried for a time on the arm or some part of the body where the hairs, though present, are few and weak.—*Lancet, July* 19, 1873, *p.* 101.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

82.—ON THE ORIFICES OF THE UNIMPREGNATED UTERUS, AND THEIR SURGICAL TREATMENT.

By Dr. J. HENRY BENNET, late Obstetric Physician to the Royal Free Hospital, London.

I will recapitulate in a few words the opinions which I entertain and teach respecting the uterine orifices.

1. *Anatomically*, the uterine cavities are two in number; the cavity of the uterus, triangular; the cavity of the cervical canal, infundibuliform. The cavity of the cervical canal is limited — externally, by a contraction of the os externum, usually patulous, and easily dilatable; internally, by a more decided contraction, which separates it from the uterine cavity, and is called the os internum. This internal contraction, I maintain, is formed by the circular fibres of the cervix, and constitutes a species of sphincter, although no positive sphincter, like that of the anus, is anatomically demonstrable. This double sphincter reproduces, anatomically, the sphincters which close the other large splanchnic cavities—the stomach, the intestines, the bladder.

2. *Physiologically*, these sphincters, especially the uterine one, are vital, close and open by a vital contraction; and a more or less vital contraction and closure is the usual condition during life. After death, this vital contraction ceases, as well as that of the sphincter ani, and must not, therefore, be looked for. These sphincters open for menstruation, probably for conception; and the internal one closes on the contact of a foreign body, such as the uterine sound. They are also more or less susceptible of contraction, according to individual idiosyncracy. Thus in a perfectly healthy woman (in the interval of menstruation) having a perfectly healthy uterus, the uterine sound or a bougie (No. 8 or 10) separates the lips of the os externum with ease, passes into the infundibuliform cavity of the cervix, where lateral motion is generally possible, and reaches the os internum, one inch and a half from the os externum, there to be arrested, generally speaking, by its vital sphincter-like contraction. Sometimes slight pressure will pass it into the uterine cavity, sometimes it will not.

Nothing short of unjustifiable force will succeed; although in these same cases a smaller wax bougie, warmed and bent, will generally pass.

3. *Pathologically.*—Such being the case anatomically and physiologically, the healthy os internum in the perfectly healthy uterus usually resisting the passage of the uterine sound or of a moderate sized bougie, it is a most egregious pathological error to consider this mere impediment to the entrance of the sound, apart from other morbid phenomena, a pathological condition warranting dilatation or division. So far from resistance to the sound at the os internum being a sign of disease, it is a sign of a healthy natural vital state of the parts. The free and easy passage of the sound into the uterine cavity, without pressure or force, is, on the contrary, generally an evidence of an actually morbid state of the organ. The extension of inflammation of the mucous membrane of the cervical canal to the cavity of the uterus, relaxes and paralyses the os uteri internum; so do endometritis itself, or any morbid state that enlarges the body of the uterus; metritis, acute or chronic; non-involution of the uterus after labour or abortion; or a fibrous or other growth developed in its tissues. Inflammatory hypertrophy, however, of the cervix may mechanically approximate, partly close, the entire cervical canal. The hypertrophy removed, the canal relaxes of itself.

Such are the views and statements which Dr. Matthews Duncan has stated to be *seriatim* wrong. I repeat that it is of the most vital importance that the real anatomical and physiological condition of women should be cleared up and firmly established as a basis of pathological work. If the above views be right, any healthy woman in the kingdom, casually examined, may be pronounced pathologically wrong, and treated for strictures which do not exist, dilated or divided—a really frightful state of things. Indeed, I firmly believe that hundreds are so treated every year, conscientiously, but under the influence of mistaken ideas as to the anatomy and physiology of the uterus. Indeed, many who have written and practised during the last few years seem to look on the uterine canals as comparable to the waste-pipe that carries off the water from the roof of a house, and to them nearly all uterine pathology is centred in fancied or real obstructions; just as another class of modern gynæcologists have got into their heads that the uterus, the most movable organ in the economy, the "animal in animale" of the ancients, is a mere joint, like the knee or elbow-joint, liable to dislocations, twists, and turns of every kind, and that nearly all uterine pathology lies in these fancied and exaggerated displacements.

Under the influence of these fallacies and errors, I am con-

stantly seeing errors in practice, cases of constitutional life-long dysmenorrhœa (or painful ovulation) divided or dilated by one, treated by pessaries left for a year or two by another, irrespectively of all constitutional considerations, and with no beneficial result whatever. The dependence of menstruation on ovulation, of ovulation on the nervous system, of the nervous system on diathesis, on hereditary and health antecedents, is often entirely ignored; and in one of the most complex branches of pathology, imperfect surgical notions are often substituted for rational treatment or non-treatment. In many of the cases thus surgically mistreated, actual lesions, important lesions, inflammatory and nutritive, are not unfrequently overlooked and ignored.—*British Medical Journal, May* 24, 1873, *p.* 583.

83.—ON THE TREATMENT OF SUPPOSED STRICTURE OF THE CERVIX UTERI.

By Dr. WILLIAM CUMMING, Edinburgh.

True stricture of the cervix uteri is rare; not so rare, certainly, as is stricture of the urethra in the male who has not had gonorrhœa, but nearly so. Incision of the cervix, or hysterotomy, therefore, for the cure of stricture, should be one of the rarest operations in obstetric surgery. But of late years it has not been so; and it may be a legitimate inquiry, what purpose it has served when the consequences seemed to sanction its use, which unquestionably they often do.

1. There may be, or rather there is often, a quasi-stricture at the internal os, when there is congestion, hypertrophy, or other disease of the lining membrane of the uterus. This condition is relieved temporarily—it may even be cured permanently—by the hemorrhage resulting from the incision. In my own experience, the relief generally is only temporary.

2. The same condition of quasi-stricture exists when there is hypertrophy of the body of the uterus—in many instances induced by the efforts to expel the clotted menstrual and the accumulating leucorrhœal discharges. In such cases, the hemorrhage, coupled with adequate general treatment, reduces the hypertrophy, prevents accumulation, and helps to restore the healthy state of the lining membrane, and so relieves the supposed stricture.

3. The preceding condition is almost invariably connected with enlargement and congestion of the ovaries, either as cause or as effect; and the same hemorrhage reduces and relieves this.

But the operation, if effectually and thoroughly performed,

is one of very considerable risk, especially when followed by the introduction of sponge or tangle-tents. The interesting mechanical, or rather dynamical, experiments of Dr. Matthews Duncan prove with what force these bodies act; and it is not difficult to estimate (if it were not sufficiently and disastrously confirmed by experience) the mischief that must in too many cases result from the laceration and irritation of the delicate textures cut into by the incision. How often this mischief has succeeded the operation, probably no one knows. Is, therefore, the operation altogether unjustifiable? I do not say so; but, as a *cure* of this quasi-stricture, it should be almost the rarest operation in surgery. Is it justifiable as a cure or relief of the conditions I have alluded to above? I believe not, for three reasons: 1. Because it does not, as a rule, either cure or more than temporarily relieve these morbid states; 2. Because it is always attended with danger; and that danger is increased the nearer the incision approaches to the internal os and the various plexuses adjoining, where the supposed need for the operation exists; 3. Because there is a much more simple, and, in my opinion and experience, less dangerous, treatment that may be employed with a larger probability of success. I cannot say utterly without risk, because every gynæcologist knows that, while there are some uteri to which almost anything may be done with impunity, there are others to which nothing can be done without startling, even fatal, results. I have known the gentle introduction of a moderate sized bougie lead to violent metritis, extending to the appendages, and not recovered from without permanent mischief; and I have also been cognisant of the same leading to acute suppuration of the ovary. But these results are so rare that, except as warnings to be both preparatory and cautious, they need scarcely be taken into account; and my present belief is, that if the gentle depletion I am disposed to advocate were employed before using the bougie, such complications would, probably, never occur.

Assuming, therefore, that true stricture is extremely rare, and that incision of the ora and cervix uteri, with subsequent dilatation by tents, is unnecessary, and may be, and often is, injurious, but that the hemorrhage resulting from it is beneficial, I have long practised, with fair success, scarification of the os uteri, followed by dilatation with graduated bougies. By scarification the amount of blood abstracted can be limited; as much or as little can be taken away as is thought necessary (which with leeches cannot be done); congestion of the lining membrane of the womb and of the ovaries is relieved and the passages are relaxed. By this means the bougie is more easily introduced; certainly requires less force in its use; does not

rouse or increase irritation of the canal and interior of the uterus, probably even soothes them: and if there be any deposition of lymph environing the cervix, facilitates its absorption.

It is satisfactory to observe that there is now a tendency to return to the more simple and less meddlesome treatment of uterine disease, and to discourage heroic modes, the details of which are sufficient to make one's hair stand on end.—*British Medical Journal, April* 26, 1873, *p.* 461.

84.—ON THE VALUE OF THE CORPUS LUTEUM AS A PROOF OF IMPREGNATION; WITH A CASE IN WHICH AN UNIMPREGNATED OVUM WAS FOUND IN THE VIRGIN UTERUS.

By Dr. WILLIAM T. BENHAM, Assistant Medical Officer, City and County Lunatic Asylum, Bristol.

Since it has become a rule in the principal lunatic asylums of this country—and in this one amongst many others—to make careful post-mortem examinations in every available case, much light has been thrown on certain pathological conditions which were before clouded in obscurity; and there can be no doubt, that much more may yet be demonstrated by the careful registration of all such appearances as may from time to time be seen by individual pathologists in their especial departments of medical science.

It is not the good fortune of many observers to meet with such a pathological phenomenon—so rare indeed as to be almost, if not quite, unique—as was seen at the examination of the body of a female patient, who died on the third day of the catamenial period, in the status epilepticus, in the Asylum for the City and County of Bristol. This case, I am sure, will be of great interest to many of your readers, throwing light as it certainly does on a medico-legal question of great importance; and I would just premise, that whatever is demonstrable by the facts here recorded, is all the more valuable from the conditions which surrounded the patient at, and for a long time previous to, her death; making it quite impossible that the pathological appearances observed in this case can be explained away by there being the slightest doubt of her having had sexual connexion with any one for at least several years before.

Charlotte S., æt. 25, single; of no occupation; was admitted into the Bristol Lunatic Asylum in December 1864, suffering from epilepsy. She died on the 26th of March 1873. During the whole of that time she was subject to periodical attacks of epilepsy with excitement, which usually came on a day or two prior to the appearance of the catamenia, which were generally of a normal character, and which ceased at their termination.

On the 22nd of March, having complained the day before of not feeling very well, she went into the status, and remained in bed in a single room, attended by a nurse, from that time until her death. A mixture of brandy, træ. sumbul., and spt. æth. sulph. (which is usually given here in such cases with good effect), was ordered to be given her every three hours, but she never rallied, and died in the afternoon of the 26th. On the morning of the 23rd it was found that she was menstruating, and was still in that state at the time of her death.

At the autopsy, which was made forty hours after death, the surface of the whole body was found to be much congested, and thickly scattered over the front of the thorax were numerous minute extravasations resembling flea-bites; there was also considerable sugillation of the depending parts; the internal organs, the brain, spinal cord, and their membranes, were all intensely congested; the case thus far presenting the appearances usually found when death has taken place under similar conditions; but the results of the examination of the generative organs were such as to make it incumbent on me to lay them before the profession, and to offer a few remarks on their physiological and anatomical bearing. I can only regret that this most interesting case did not fall into abler hands than mine, so that a more elaborate account might have been given of it, and thus have made it of more value and interest than any attempt of mine can do.

On examining them externally, the organs of generation presented in a marked degree all the highly characteristic signs of virginity. On removing the uterus, ovaries, and Fallopian tubes, they were found to be in a state of intense congestion, the superficial veins standing prominently out, and being filled with dark-coloured blood. The left ovary was more congested than the right, and on its upper anterior surface, situated rather more internally than externally, was seen a bean-like prominence, surrounded by a plexus of blood vessels all the more apparent from their congested condition; it was of a polished appearance, and of rather a bluish shade of colour. On examining this body it was noticed that its upper surface presented a small nipple-like prejection, apparently formed by the distension of a small portion of the peritoneal covering of the ovary, and containing some soft distending material; some small blood vessels could be observed ramifying round the base of it. At the centre of this prominence was a small irregular dark spot as if a small orifice had existed there, and had been closed up by a clot of blood. On making an incision through its long diameter, there was at once seen an oval shaped cavity filled by a mass of partially decolorised fibrin, and entirely surrounded—except at the point corresponding to

the nipple-like process—with a thick yellow substance of considerable firmness, pierced by a number of small blood-vessels, which sprang from the vascular plexus surrounding it externally; and on using a magnifying-glass of low power, these minute vessels could be seen traversing its substance, and losing themselves on its inner edge, which was contiguous to the surface of the partially decolorized fibrinous clot filling up the cavity. On measuring this cavity it was found to be five-eighths of an inch in its long, and three-eighths of an inch in its short diameter. The yellow substance surrounding it was found to be of the uniform thickness of one-eighth of an inch, the whole structure measuring seven-eighths of an inch in its long and five-eighths of an inch in its short diameter. On again examining this yellow substance, it was seen to be of a firm granular-like matter, with many minute oil-globules scattered over its surface; and in parts where the blood vessels were the most numerous, they gave it a pinkish yellow appearance. It will be at once seen that we have here occurring in the virgin ovary a corpus luteum, possessing such decided characteristics as to make it *per se* quite indistinguishable from the so-called *true* corpus luteum of impregnation of the same period, asserted by many writers—Paterson, Lee, Montgomery, Bernand, Deschamps, Müller, Ramsbotham, and others—never to occur in any but in the ovary of an impregnated female, and consequently regarded by them as an infallible proof of a recent pregnancy.

Hoping almost against hope, that the ovule, which had evidently been only very recently extruded, might still be found in the cavity of the uterus, a section was made through its anterior wall, and, on laying it open, there was seen a small circular body, of a reddish-white colour, almost buried in the pulpy purplish-coloured decidual débris, which thickly covered the whole of the anterior surface of the uterus. On carefully removing what I could of this decidual matter from its surface, and examining it with an inch lens, it appeared to be more of a pinky-white colour, and having a fine velvety surface, which, under a higher magnifying power, was seen to consist of multitudes of white filaments. It measured, with the decidual matter surrounding it, and which could not be entirely separated from it, one-twentieth of an inch in diameter, and was situated a little more than seven-twelfths of an inch above the os internum, and slightly to the right side of the median raphé of the uterus. As I have already said, under a higher power its surface was seen to be composed of very short white filaments, which gave it much the appearance, only on a smaller scale, of one of the earliest impregnated ova on record, discovered and described by Velpeau as measuring

about five-twelfths of an inch in diameter, and the filaments of which were so far developed as to measure one-twelfth of an inch in length. This ovum he stated to be fourteen days old.

On making a section through this ovule, with the object of examining it more minutely, I noticed that a very small quantity of albuminous fluid escaped from it, and it lost its globular form directly. A partially collapsed cavity of very minute size was seen to occupy its centre; and although I carefully examined the excised ovule and the fluid which had escaped from it, I could discern nothing more of importance.

It would be interesting to know at what time this ovule was extruded—before, at, or on the day after the time of the commencement of the catamenial flow? Whether it underwent the same external changes in its passage to the uterus as the impregnated ovum? Whether, irrespective of impregnation, an ovule in its descent usually takes on the velvety appearance above described? How long it remains in the uterus before passing away? and whether it passes off in the menstrual fluid entire, or in a state of dissolution? Whether the ovule theory—that at each menstruation an ovule is discharged, as in this case—is capable of demonstration by facts which are as yet undiscovered in the human female? One solitary case like this will not go far to answer any of these queries; but it does prove this much, that an unimpregnated ovule can and has descended into the cavity of the uterus, and remained there for some days without being, as stated by some, immediately washed away with the menstrual fluid, or without, as stated by others, having undergone such rapid dissolution as to have immediately passed away; and, what has been denied by many, that an ovule is ever impregnated in the cavity of the uterus itself, is now made extremely probable. There can I think be no doubt that, had this girl had sexual connexion, this ovule might certainly have become a fecundated ovum; unless, indeed, which has never yet been demonstrated is the case, that the menstrual fluid, through which the spermatozoa would have to pass to reach the ovule, possesses the power of destroying those bodies immediately on coming into contact with them. Before passing from this subject, let me state that I think this individual case goes a long way to make it probable that where an ovule is extruded it takes place at the commencement of menstruation more frequently than at any other time. This ovule had evidently been extruded for some time—that is, not less than two days; and that it had not been extruded immediately before death is evident from the fact of its having been found in the cavity of the uterus, embedded in the decidual débris. It must, there-

fore, have taken some time to have been conveyed there, considering the distance it had to travel; and, besides, the condition of the fibrinous clot filling the cavity it left makes it certain that it had been extruded at least two days previously. It appears, then, that in this case the ovum was extruded at the commencement of menstruation; and that it is usually so in other cases seems, I think, to be indicated by this, that at the time of, and for a day or two previously to menstruation, a much greater supply of blood flows to the ovaries than at any other period; and the fact of the distended Graafian vesicle sharing in the increased vascularity of the whole structure at that particular time, makes it extremely probable that the extra pressure then put upon the captive ovule should be sufficient to cause it to burst its coverings and to become extruded, and should it not be sufficiently developed to do so at that period, in all probability it would not become extruded until the ovary should be again the subject of increased vascularity; and this seems to show the reason why a menstrual period often passes without the extrusion of an ovule at that particular time. It would appear, then, that the increased vascularity which takes place at the menstrual period goes a long way to explain the mechanism by which an ovule is extruded more frequently at that than at any other time; also, there can be no doubt that the increased vascularity is of great importance in producing those changes in the Fallopian tubes and their fimbriated extremities, which result in those parts of the sexual apparatus taking on the turgid and erectile character necessary for the due performance of the function of grasping the extruded ovule and of conveying it to the cavity of the uterus. But where, in cases by no means uncommon, an ovule is extruded and becomes impregnated at an *inter-menstrual* period, I am bound to believe, from instances which have come under my own observation, that it is owing to the fact that, from some exciting cause more or less potent, the ovaries have taken on an increased vascularity of action resembling that incident to menstruation, and sufficient to extrude a well-developed ovum at an abnormal time. Thus, the pleasures and excitement of the opera or the theatre, of the "mazy dance" and supper rooms, of the merry dinner-party, or of the return home after a prolonged absence, have doubtless often been the cause of that increased rush of blood to the ovaries and Fallopian tubes, which has resulted in the extrusion and possible impregnation of an ovule at an inter-menstrual period. In support of this I may just mention a case which has occurred since the foregoing was written, affording considerable negative evidence that such is the case:—E. T., aged 26, an epileptic, died under precisely similar con-

ditions to the last patient; but although her death happened on the fourth day of menstruation, her uterus, ovaries, and Fallopian tubes presented only very slight, though sufficient evidence of such being the case; there was only a small degree of increased vascularity, and on examination it was quite evident that no ovule had been recently extruded, although the right ovary contained a Graafian vesicle of such mature development, that it was easy to conceive an ordinary amount of ovarian vascularity must have resulted in its extrusion; but the fact that only the very slightest degree of congestion was present would sufficiently seem to indicate the true cause of the non-extrusion of the ovule in this particular instance. I before suggested that the non-extrusion of an ovule might be, and probably was, due to the state of the development—or rather of the undevelopment—of the ovule itself; but it would now seem that the *amount of ovarian vascularity* has also much to do with it: for in this case, although there was a fully ripened Graafian vesicle on the surface of the ovary, the amount of congestion was too slight to have any effect sufficient to cause the extrusion of its ovule. I would also add that there were the remains of two former corpora lutea, well marked in character, the one more so than the other; and I think it extremely probable that they were the result of the two previous menstruations, which I ascertained were of a severe character, and accompanied by considerable proof of a well-marked vascular disturbance.

Let me now pass on and state, in as few words as possible, the bearing this case has on the question of the value of the corpus luteum as a test of impregnation.

It has been observed by Dr. Dalton, that the corpora lutea following menstruation and impregnation, for some time take the same course of development; and that there is the same rupture of the vesicle, the same extravasation of the blood, followed in either case by the gradual absorption of the colouring matter of the clot, and hypertrophy of the membrane of the vesicle; and thus far he agrees with those who look on the sign of the so-called true corpus luteum as fallacious; but then he goes on to state "that the corpus luteum of impregnation continues to develop itself, and does not become decidedly retrograde until the termination of the pregnancy, whereas the corpus luteum of menstruation reaches its maximum of development at the end of three weeks, and then rapidly becomes atrophied." There is, however, considerable doubt on this point, and it is by no means clear that such is often the case. Bischoff, amongst many other observers, may be quoted as believing that the so-called true corpora lutea may be produced independently of impregnation, and that

they consequently cannot be received as proofs of pregnancy. Casper believed that the corpora lutea of menstruation "were in no way distinguishable from those of impregnation;" and Kirkes states that similarity in the character of the two kinds of corpora lutea "necessarily renders unsafe all evidence of previous impregnation founded on the existence of a corpus luteum in the ovary."

That these authorities are right in their conclusion, this case, I think, materially helps to prove. The fact of a corpus luteum of menstruation having been found to so exactly imitate those of impregnation, if, indeed, this was only a *solitary* instance, must considerably weaken the theory still held by many, that a so-called true corpus luteum is a sure sign of impregnation. When we come to consider why impregnation should make such a vast difference in the appearance and structure of the corpus luteum resulting therefrom—as is said to be the case—it is by no means easy to see the reason why such should be so.

If it could be proved that in every case in which impregnation takes place, it does so whilst the ovule is still in its vesicle in the ovary, and at no other time or place whatever—and this, connected with the fact that the corpora lutea in the impregnated female as a rule possess such decided characters—it would be only fair to suppose, although, perhaps, even then difficult to demonstrate with our present knowledge of the subject, that the proximity of the fecundating element which causes such vital changes in the ovule itself, might have some sufficiently potent action on the structure immediately surrounding and in actual connexion with it, to account for the characteristic changes they are known to undergo. But that it is not the case that fecundation takes place in the ovary alone is sufficiently proved by the fact of the unimpregnated ovule having been found, as in this case, elsewhere than in the ovary, at the same time possessing all the highly characteristic features which distinguish it, when it is examined, in the Graafian vesicle itself, and therefore quite as capable of undergoing fecundation when brought into contact with the necessary elements. Granted, then, that the ovule may become fecundated away from the ovary itself, we cannot suppose that the fecundating material in that case could have any direct material action in the formation of the corpus luteum, which would, in that particular case, be found to exist; but under such circumstances the resulting corpus luteum, not having come into contact with the fecundating material, would certainly have the same formation as it would have did fecundation not take place at all. Can we, then, be surprised that, subject to the same early conditions—subject to the same absence of

contact with any fecundating material—the corpus luteum of the ovule, which has passed on into the uterus, and has only received impregnation after remaining there for some time, should exactly resemble the corpus luteum of the ovule, which has passed in the same way to the uterus, but which in the end has not become impregnated at all? We must, therefore, conclude, granting the absence of the fecundating element in both cases, that there is no reason why the corpus luteum of menstruation should not exactly resemble the corpus luteum of impregnation in all such cases as come under the conditions I have named. In fact, it does so, as I hope I have sufficiently demonstrated; and, consequently, it follows that the presence of a so-called true corpus luteum, of at all recent formation, in the ovary of a female, has not the slightest legal value whatever in determining the question as to whether impregnation has taken place or not. If that is proved, as I believe it is, by the facts I have brought forward, it should be of considerable importance in its medico-legal as well as in its anatomical and physiological bearings; for, in one case at least recorded by Dr. Guy, the existence of a corpus luteum was held to *prove* that conception had taken place, when the uterus itself presented not the slightest signs of such having been the case.

Much more might be written on the facts I have endeavoured to bring forward, but so much has at various times been said on the subject, that it has not been my object to go over that which has, from time to time, been written by others; but rather to produce such new facts, valuable in themselves, as my case demonstrates, with the hope that they might, instead of being buried in obscurity, be of service to those who make this and allied subjects their especial study.—*Edinburgh Medical Journal, Aug.* 1873, *p.* 127.

85.—A SAFE METHOD OF INDUCING PREMATURE LABOUR.

By Dr. BEVERLEY R. MORRIS, Nottingham.

[Supposing it decided to bring on premature labour in any given case, what is the best method of effecting it? No doubt the nearer the natural process of parturition can be imitated the more successful will be the result.]

The process that I have to describe is an adaptation of the third method of inducing labour—that is, by galvanism; but, as far as I am aware, by an entirely different application of the principle from any before attempted. The principle involved

was introduced by Mr. Dancer of Manchester many years ago, for the purpose of arresting *post partum* hemorrhage, and this it undoubtedly effected satisfactorily: but the apparatus was so cumbersome, that few practitioners could carry it about with them, and, probably from this cause, I do not think it was ever generally used. The instrument invented by Mr. Dancer was so arranged that one pole of the galvanic current could be introduced into the uterus, while the other was applied over the abdomen; it was so constructed that either a continuous or an interrupted current could be applied. The instant effect was a powerful contraction of the uterus, and a consequent cessation of hemorrhage.

The application of this principle to the induction of premature labour was made by Mr. John Varley, surgeon, of Nottingham, through whose kindness I am enabled to exhibit the instrument as modified by him. The mode of using this instrument is to insert the metallic point within the os uteri, and then, placing the other pole to the abdomen, pass a slight continuous current through the uterus for ten minutes or a quarter of an hour. This induces a dilatation of the os, which is further increased by substituting a larger conical point, and again continuing a gentle current for a few minutes. In each case in which this method has been used so far, labour has followed in two or three days; but, should this not be the case, it will only be requisite to apply the current daily until it does. The safest way is to expose the os uteri by a speculum, and then insert the point of the instrument through the speculum, which may then be withdrawn over the instrument. The great portability of the instrument and battery will allow it to be readily carried in the pocket, and it is always ready for use at a moment's notice; and the induced current seems to me amply sufficient for the purpose. The arrangement for giving a continuous or broken current is very simple, and entirely and instantly within the operator's power.

The instrument consists of a metallic sound, covered, except at the point, with a non-conducting material, and having a metallic connection at the handle, and so arranged as to be either broken or continuous by a touch of the finger. This intermediate part is connected with one pole of the battery; while the other pole is attached to a metallic tube or conductor for external application, either direct, or through the hand of the operator. The extreme portability of Gaiffe's battery led Mr. Varley to this adaptation, which, I trust, will soon be widely used, and lead to a safer and more efficient method of inducing uterine contraction than any at present in use. It is manifest that there are other cases in which it may be most

usefully applied, as, for instance, in sluggish or atonic labour, and other similar states.—*British Medical Journal, Sept.* 6, 1873, *p.* 276.

86.—WILL IT BE A BOY OR A GIRL?

[We extract the following from Dr. Braxton Hicks's address in the section of Obstetric medicine at the meeting of the British Medical Association in London last autumn.]

The interesting fact that the sex of the child can in a large proportion of cases be ascertained by auscultation during pregnancy has also lately been made known. When the fœtal pulsations number 144 per minute, the child is probably a female, when they are 124 per minute, probably a male. It is said that a little variation from 124 upward, and from 144 downward, will not alter the diagnosis, provided auscultation be practised towards the end of pregnancy. Steinbach was correct in forty-five out of fifty-seven cases which he examined, while Frankenhäuser was right in all the fifty cases which he examined with a view to determine the sex of the child.—*British Medical Journal, Aug.* 16, 1873, *p.* 187.

87.—ON THE COMPARATIVE ADVANTAGE OF FORCEPS, OF TURNING, AND OF PREMATURE LABOUR IN CONTRACTED PELVIS.

By Dr. ANGUS MACDONALD, Lecturer on Midwifery and Diseases of Women.

Before attempting any statement of opinion regarding these points, let me briefly go over the clinical history of two women, whose cases are brought forward more as a theme from which to start the discussion, than as data on which to found deductions.

Case 1.—That of Mrs. D. is peculiarly interesting. She is at present in exceedingly good health and spirits, buoying herself up in the hope that her next labour may be twin-boys, as her mother had twins once. Should this occur, she believes she may yet have a living boy, which is an object of great ambition with her. Her age is 41. She was married on the 10th of March 1854, since which time she has given birth to nine children at full term, to two at between 7½ and 8 months, and has had three abortions, which make in all 14 conceptions. She is of middle size, strongly made, and apparently well formed. Her pelvis, however, is very much contracted in the conjugate diameter of the brim, though roomy at the sides, and also roomy and regular at the outlet. The narrowing is

effected by jutting forwards of the promontory of the sacrum, and is such as to contract it to close upon three inches, so far as one can judge. The account I give of her confinements is, with the exception of the last three labours, obtained from her own statement, so that if there are mistakes in regard to any of them, some of the members of this Society, who happen to be referred to by her, will, I hope, put me right.

Her first confinement took place on 19th December 1854, when she was delivered of a female child. She was three days and three nights in labour. On the third day it was agreed upon between Dr. Pattison, who had charge of her case, and Drs. J. Y. Simpson and Sidey, to meet at 7 p.m., with the view of effecting delivery, provided the natural efforts were not successful before then in terminating the labour. She, however, gave birth to a living child, unaided, an hour before the appointed meeting. Her recovery was very slow, but complete. The child lived for several years, and died of hooping-cough.

This pregnancy was followed by two abortions. Her second confinement took place on 2nd February 1858. Dr. Sidey attended her. She lingered for a whole day, and Dr. S. waited with her all night, till between two and three o'clock in the following morning, when Dr. Keiller and he having chloroformed her, first attempted to deliver by instruments, but finally completed the labour by version; the child was a female, and is still living. Her third confinement occurred on 17th June 1859. Dr. James Sidey, along with the late Mr. William Scott Carmichael, attended. She was delivered by turning, under the influence of chloroform. The child was a male, and dead.

She was delivered by turning, under chloroform, of her fourth child, a girl, and living, by Dr. James Sidey, on 19th March 1861. In the end of this year she had an attack of bilious fever, and in January 1862 was delivered of a female child, at about the seventh month. This infant survived only nine hours. The delivery was easy, and unaided. On 29th January 1863, Dr. Andrew Inglis, assisted by Dr. James Sidey, induced labour at the eighth month. In this case neither instrumental assistance nor turning was needed. The baby was a female, and though always weakly, lived till she was eighteen months old, and died in teething. The mother disliked the necessary interference, in connexion with the induction of premature labour, so much that she ever since declared that she would rather die than again submit to the same ordeal. In her seventh pregnancy she was allowed by Dr. Andrew Inglis to go on to the full time, chloroformed, and delivered, by turning, of a female child, which lived. This hap-

pened on the 6th December 1865. On the 23rd of May 1867, Dr. Andrew Inglis again delivered her of a living female child, at full term, by version and chloroform.

On the morning of the 29th of June 1869, I was hastily summoned about 6 a.m. to attend her. She had previously insisted that she must be delivered by turning, and as Dr. Andrew Inglis, who had handed over the case to me, was of the same opinion, it was agreed that on this occasion I should deliver her by turning. The os uteri was completely dilated, and the water had escaped about half an hour previously to my arrival. The pains were excessively severe, yet the head had never entered the pelvis, and in the interval between the contractions was quite movable at the brim. I chloroformed the patient, turned with no difficulty whatever, and delivered her of a male child, which, however, never breathed. The promontory of the sacrum left a deep indentation on the child's head. Great force was needed to bring the head through the brim. Mrs. D. made a speedy recovery.

On the 22nd of October 1870, I was again called hurriedly to attend her. I found her in labour; the os fully dilated, the pains severe, but the membranes unruptured. I accordingly chloroformed the patient, and was proceeding to effect delivery by version, but on introducing my hand through the membranes, I found I had to deal with an ill-developed fœtus floating in an excessive collection of amniotic fluid. On allowing this to escape, a small male fœtus was expelled, which made a few faint efforts at breathing, and then died. On the 2nd of January 1872, she had an abortion at about the third month.

On the morning of 28th of January 1873, she was delivered by me by turning, under chloroform, of a living female child. On this occasion also great force was needed to pull the child's head through the brim. The side of the child's head, which looked posteriorly, was deeply indented by the sacrum, and the consequent depression on the skull still remains to a certain amount. On this occasion I must confess that I was surprised as well as delighted when the child gave indications of life. The infant remains a healthy strong child.

Case 2.—Aged 25, was seized with labour pains on the forenoon of 22nd March 1871. My then pupil, now Dr. Shapter, of Exeter, sent for me late in the evening. On my arrival I found the os completely dilated, the pains very severe, but the head still stood over the inlet, and refused to enter the pelvis. The sacral promontory was very easily reached, and the whole pelvis—as, indeed, the woman's whole person—was remarkably small. After a little consideration, I thought that I should best succeed in effecting delivery by turning. Version

was effected ultimately, but with some difficulty, owing to the spasmodically contracted condition of the uterus. The child had to be removed with the exertion of great traction force, and it took me nearly half an hour of interrupted pulling before I got the head clear of the pelvis. On an examination of the child after delivery, it was found that the connexion between two of the cervical vertebræ had yielded during the extraction. The mother made a good recovery.

From the want of success in this labour, it was resolved, with the patient's consent, to endeavour to bring home a living child in the next pregnancy by inducing premature labour. In this case my then pupil, now Dr. Oswald Wood, had the general charge of the case. A condensed abstract of his report of it is as follows: "It having been deemed proper to induce labour at about seven and a half months of pregnancy, on the evening of 20th April 1872, Dr. Macdonald began the dilatation of the cervix by means of Barnes's gutta-percha dilators. Numbers 1 and 2 were introduced successively, and so far the dilatation advanced normally. But after the removal of No. 2, the cervix threatening to become spasmodic, it was deemed inadvisable to proceed to No. 3, and after introducing a flexible catheter between the uterus and the membrane and leaving it there, the case was left till next evening. At that time Dr. Macdonald, accompanied by Dr. Affleck, succeeded in effecting nearly complete dilatation of the cervix by No. 3 dilator.

"Pains by this time became established, but were much more of an irritating than of an expulsive nature; and as the head was, after waiting for an hour or two, making no progress, delivery was finally effected by the long forceps. The child came round and cried loudly, but died in about six hours. The patient on this occasion also recovered excellently."

This woman again becoming pregnant, and having a distinct aversion to a repeated attempt at saving her child by premature labour, I resolved to wait till the full term, and then attempt to deliver by forceps, leaving craniotomy or cephalotripsy as the alternative in case I failed with forceps. Accordingly, the patient having been seized with labour pains at full term, on the 4th May 1873, she was attended by my pupil, Mr. John Bisset Smith. She had weak pains throughout the day, but these became exceedingly severe between seven and eight o'clock p.m. The membranes had ruptured immediately before ten p.m., at which hour I was called to attend her. I found the head presenting at the brim of the pelvis, with the sagittal suture approximating the transverse diameter of the inlet, with the occiput towards the left side of the mother. The head at this stage exhibited well-marked

Roederer obliquity. At half-past eleven, as the head was making no progress, although the pains were long, strong, and frequent, and the cervix completely dilated, fearing rupture of the uterus in case of further delay, I applied long forceps, and after a pretty tight pull for the space of a quarter of an hour, succeeded in effecting delivery. The forceps were found to have been so applied that one blade was over the right eye, one limb of the blade being in a line with the ridge of the nose, whilst the other blade had become fixed behind the left ear, which proved the truth of the observation in regard to the transverse position occupied by the head at the brim. The child, a large, well-developed female, was semi-asphyxiated, but soon began to breathe under the ordinary restorative applications. The placenta, along with some clotted blood, was speedily expelled by the natural mechanism. There was for some time slight hemorrhage. The patient and her child both did well.

The conjugate diameter was, on the strictest examination, made both by Dr. Affleck and myself, after her second confinement, determined to be about $3\frac{1}{4}$ inches; the diagonal conjugate, $3\frac{3}{4}$ inches.

The first remark I am inclined to make in reference to these cases is, that it seems to me that we are too apt, in Great Britain, in deciding as to the preference of one operation over another, to fix our attention exclusively upon the amount of contraction in the antero-posterior diameter, without, at the same time, keeping before us the presence or absence of contraction of the other diameters of the pelvis. Though there is no doubt that, generally speaking, though not always, the conjugate is the narrowest diameter in deformed pelves, yet, according as we have a contraction in the antero-posterior diameter merely, or general smallness of the pelvis, with or without a marked contraction of the sagittal at the brim, the operation of turning is followed with very different degrees of success. Now, the pelvis, in Case No. 1, presents to my mind a good example of the transversely flattened pelvis, roomy at the sides, but markedly contracted in the antero-posterior diameter of the brim. Such pelves, as they afford a considerable amount of room for the occiput to occupy in one or other of their sides, present favourable conditions for successful turning, while, at the same time, the narrow conjugate renders them somewhat less suitable for the application of forceps. In confirmation of this statement, it is to be observed that of all the three operations of forceps, premature labour, and turning, the latter gave the best results in this case. But No. 2 being one of generally contracted pelvis, possibly, however, or rather probably unequally so, was very unsuitable for turning, whilst

it gave an opportunity of effecting good results by forceps, and perhaps also better by waiting a considerable time before applying the instruments. The view that this pelvis belonged to the class of generally contracted pelvis, is made evident from the extreme flexion, or Rœderer obliquity of the head, which was noticed in the third confinement. Such pelves are exceedingly ill suited for delivery by version. This is prominently pointed out by Borinsky, Archiv für Gynäkalogie, B. iv. s. 230. That British authorities, although no doubt aware of this fact, do not point it out with sufficient clearness, is, I think, unfortunate. At any rate, it was not sufficiently prominently before my mind three years ago, else I would certainly have preferred forceps to turning in the first confinement of Mrs. N.

It is also very curious to notice how small an increase of the size of the child's head is found to be an obstacle of the most serious nature in cases of contracted pelvis. It will be observed that of Mrs. D.'s children, not a male was able to be brought to the world alive,—the great variety in her medical attendants may fairly be held as eliminating all specialty in respect to relative skill in treatment,—whereas almost all the female children lived.

And now, a few words on the general proposition of this paper. In regard to the operation of premature labour in connexion with pelvic deformity, two very able statistical papers have lately been published in Germany by Spiegelberg and by Litzmann, in the first of which it is broadly asserted that artificially-induced premature labour gives, in contracted pelvis, on the whole, very much worse results than allowing the cases to be terminated by the unaided natural efforts.

The results of Spiegelberg's collection of recorded cases of labour in contracted pelves are decidedly against the operation. The fatality in cases of contracted pelves generally he finds to be 6·6 per cent. of mothers, and 28·7 per cent. of children. The mortality, on the other hand, in artificially-induced premature labour generally, he finds 18 per cent. of mothers, and 66 per cent. of children; whilst the operation of premature labour undertaken for narrow pelves only gives a resulting mortality of 15 per cent. of mothers, and 66·9 per cent. of children. The percentages embrace a very large number of cases, from a wide circle of operators; the total number of labours with narrow pelves being 1224; of artificially induced labour for all causes, 271; of the same for narrow pelves, 219.

Litzmann, in the elaborate article already referred to, is inclined to believe that the results of carefully-selected and well-managed cases of artificially-induced premature labour ought not to prove so fatal to the mother as Spiegelberg makes out. But that the fatality to the children, taking into account

not merely the number born dead, but the number who die within the first week or two after artificial premature labour, is quite as great as Spiegelberg makes it.

Of 373 cases of contracted pelves recorded by him, and variously treated, 6·9 per cent. of mothers, and 20·3 per cent. of the children died. Of the same cases, 34 of artificially-induced premature labour gave a maternal mortality of 14·7, and a fœtal mortality of 55·8. He divides deformed pelves into four grades of contraction. His *first grade* of contraction includes—simple flat pelves with conjugate = 3·7—3·3 inches; generally narrowed, flat pelves with conjugate = 3·9—3·6 inches. Of these he records 316. Total mortality of mothers, 5·06; of children, 12·6. From artificially-induced premature labour, of which there were two cases, the maternal mortality was *nil*, but one of the children died = 50 per cent.

His second degree of contraction embraces—simple flat pelves, conjugate = 3·3—2·9 inches; generally narrowed, flat pelves, conjugate = 3·6 inches. Of 47 labours with pelves of these dimensions, the total mortality of the mothers was 10·6 per cent.; of the children, 61·7 per cent. Those deliveries effected by premature labour artificially induced gave a mortality of mothers 7·4, of children 51·8.

Classifying still according to degree of pelvic contraction, his results from 8 labours with pelves contracted to his third degree, which embraces—simple flat pelves, and generally contracted, flat pelves, conjugate = 2·9—2·2 inches. Total mortality of mothers, 50 per cent.; of children, 75 per cent. Of 5 cases of artificially-induced premature labour, 2 mothers died, or 40 per cent., and 4 children, or 80 per cent.

Of his fourth degree of contraction, which embraces pelves with a conjugate of 2·2 inches and under, it is not necessary to speak. Litzmann records only 2 cases, 1 in which delivery was effected by cæsarean section, and mother and child were lost; and another in which spontaneous premature labour came on, mother living and the child being lost. It would appear, however, from the statistical results both of Spiegelberg and of Litzmann, that spontaneously originating premature labour gives considerably better results in contracted pelves than when it is artificially induced. This result is obviously referable to irritation of the mother's parts by the various methods employed to bring on labour. It seems also evident to me that Litzmann's habit of practising, more particularly the method of intrauterine injection of fluid, has something to do with several of his unfortunate cases. Litzmann concludes as follows:—"The results of my investigations, as bearing upon practice, I should wish briefly thus to formulate. The artificial induction of premature labour in narrow pelves is offered es-

cially in the interest of the mother; the saving of the child by its means is at least doubtful. The operation is, even in primiparæ, indicated in the second degree, and, at most, just at the commencement of the third degree of pelvic contractions" (*i.e.*, from 3·3—2·9 inches in conjugate); "in the first degree (3·9—3·3 inches conjugate) it can only be defended through the existence of specially difficult relations and complications."

It will thus be seen that while Litzmann—than whom no obstetrician in Europe is better entitled to speak in a matter involving contracted pelves—is of opinion that premature labour may give the mother a better chance than doing nothing, so far differing from Spiegelberg, yet the fœtal mortality is simply enormous. Now, as premature labour has always been argued for, most of all on the ground that it gave the child a better chance, without bringing additional danger upon the mother, it seems to me that Litzmann's apology in its behalf is rather weak. There is a general impression throughout the profession that premature labour ought now to be much more seldom induced than formerly. I think it is a right one; and, from such records as I have brought before the Society, it seems to me that it were better for the mothers, and much better for the children, that the operation in connexion with deformed pelvis were altogether given up. I do not profess to speak from much personal experience, but from reading and oral evidence. I have seldom found cause to employ it. This I do not regret; for, so far as all the information I can gather from reading and otherwise goes, and from the little practice I have had in it, it seems to be proved incontestably, that on very few occasions indeed is it the preferable operation. In one of the cases recorded by me, a weakly child was born, and lived some years, but succumbed to teething. I am not sure but in such, and in many other cases of death from children's disease, we are right in following the views brought before the profession lately by Virchow—that congenital weakness has much more to do than we usually think with such fatal results. Thus, a child is said to die of hooping-cough, of measles, of teething, or of bronchitis ostensibly, but it really dies of congenital weakness, or of congenitally defective development. In the other instance, the infant survived its birth only a few hours.

But now, to turn very briefly to the comparative advantage of turning by the foot as an alternative to premature labour or forceps. This was argued to be, in reference to long forceps, a preferential operation by the late Sir James Y. Simpson, with his acknowledged ability and acuteness. Schroeder has also lately expressed himself as strongly in favour of the operation in the interest of the mother, especially, in contracted pelvis, even down to 2 inches 8 lines of conjugate. Borinski, in the

paper above cited, has discussed this question at great length, and with much ability. He points out with great precision the danger likely to arise from attempts at delivery by turning in generally contracted small pelves, and restricts it to the simple flat contracted pelves, and considers turning is more particularly required when the head presents at the deformed brim with lateral obliquity of the neck, so as to make the posterior or anterior parietal bone present with the saggital suture running transversely, either forwards near the symphysis, or far backwards near the promontory of the sacrum.

My two cases illustrate the truth of Borinski's view. Case 2 being one of generally contracted (flat?) pelvis, was of the worst shape to be benefited by turning. The fatal results of turning to the child, and the successful results of forceps, point, I think, to what ought generally to be our practice on these occasions. There must be in the recollection of every obstetrician of considerable experience a good few cases in which he had prepared his craniotomy or cephalotripsy instruments, and found, on applying forceps, that after a period of careful but vigorous traction, his efforts have been rewarded by a living child and a healthy mother. On the last occasion when Mrs. N. was confined, I carried my cephalotribe to her house, in case forceps should fail. I well remember also another case in which I attended along with my then pupil, the late lamented Mr. Thomas A. Peddie. It was that of a girl married at 16 and pregnant at full term, before she was out 17. On this occasion there was no deformity, but the pelvis was altogether too small, and the child was large. I waited a whole night and a day, but though the os was fully dilated, the head would not engage in the brim. I took with me also on this occasion my cephalotribe, &c., but determined to give the child a chance by forceps. To my astonishment, after a great deal of tugging, a living child was born. It did well, and so did its mother. Such cases are cropping up every day.

The cases in which turning is found to succeed best are such as my first case, in which we found on the whole that it did very well. But the great majority of such cases are made up of second and subsequent cases, which proved in the primiparous condition excessively difficult, and led to craniotomy and other serious results, as it is seldom in a first case that a difficulty suitable to be overcome by turning is proved to be so in time to allow of this operation being performed. These cases, however, are found to terminate just as easily by the application of forceps, if not without any interference at all. It is quite clear, on reading Schroeder's note after his paragraph on turning in deformed pelvis, that his views are arrived at

chiefly from cases of great difficulty in first labours proving easy in subsequent labours, when turning was employed. But I hold that this reasoning is purely the *post hoc* fallacy. In the earlier part of my practice I turned much more frequently than now in such cases, and on the whole got fairly good results. Now I interfere much less readily, and find the results far better. I am not, however, inclined to hold that turning is such a purely harmless operation for the mother as Schroeder and others maintain. I can look back to two occasions on which I regret, for the mothers' sakes, that I did not employ forceps instead. I must also protest *in toto* against the views of Borinski, which are also to a certain extent shared in even by Schroeder and others, respecting the danger incurred by mother and child in using forceps. Such serious results may be true in employing those German and French instruments that are too long and approach too closely at the points, but are certainly not to be dreaded from the careful use of the English instrument, which is light and little bulky, and whose blades at their distal extremities do not approximate within an inch when out of use, and are much wider apart when applied.

Another misconception seems to have possessed men's minds in reference to the effect of the application of the forceps in increasing the tension of the already too small conjugate. Obstetricians persist in ascribing to the forceps the power of being a compressor. Now, every one possessing a knowledge of mechanics sees at a glance that the amount of pressure which is needed to prevent the forceps from slipping is very little indeed, and seldom, if ever, such as to interfere so injuriously with the progress of the head as Simpson and Schroeder will have them to do. If evidence on this score were needed, a single glance at such an instrument as Professor Inglis's long forceps, which I know to work well, ought to convince the most sceptical. The forceps is essentially a tractor, and nothing else, when properly applied and used, being able to exert the necessary traction with the slightest amount of compression, and mainly by friction of the blades on the child's head.

But without further discussing this question at present, I may conclude by stating that, so far as my own views of practice lead me, they are as follows:—

1. That, looking at the statistics of Spiegelberg and Litzmann, it seems exceedingly doubtful if the operation of inducing premature labour ought ever to be employed in cases of contracted pelvis.

2. That turning does not present any proved advantage to the mother over long forceps in cases of contracted flat pelvis (so far at least as my experience and my reading enable me to

form an opinion), and is undoubtedly more dangerous to the child. That it is entirely unsuitable when the contraction is general, being much more dangerous to the mother than long forceps, or any of the higher operations.

3. That in contracted pelvis, as a general rule, it is on the whole safer to let the case go on to the full term of utero-gestation, and then give the patient a fair trial, so as to ascertain what nature is likely to accomplish unaided, without waiting so long as to allow the mother to run unnecessary risk. Then, in case there is room for the introduction of forceps, they ought to be applied and delivery attempted by their means. If this is impossible, then delivery ought to be effected by cephalotripsy, craniotomy, or Cæsarean section.—*Edinburgh Medical Journal, Sept.* 1873, *p.* 214.

88.—ON THE USE OF THE ELECTRO-MAGNETIC CURRENT IN THE SECOND STAGE OF LABOUR.

By Dr. A. E. M'RAE, Penicuik.

[The cases which form the substance of the following paper occurred in 1868 and 1869. The words electro-magnetic and electro-galvanic are used as practically well-nigh synonymous.]

It is well known that galvanism is a powerful agent in exciting muscular contraction. The uterus being a muscular body is susceptible of its influence. Some high authorities in gynæcological science, reasoning more from analogy than from actual experiments, have given their opinion against it. Later and more extended observations, however, have shown that galvanism, under certain conditions, has a powerful influence over uterine action. The following cases will, I think, in some measure support the theory:—

Case 1.—Mrs, S., aged 28, a well-made woman of about 5 feet 6 inches in height, was in labour with her second child; she had been in hourly expectation of the birth for the previous twelve hours. I was called about eight o'clock in the morning. I found the os well dilated, and quite flaccid, as were also the vaginal walls and perineum. The pelvis was well formed, and there existed no mechanical impediment to the progress of the labour. The bowels had been well opened on the previous day. The tongue was slightly whitish. The pulse was soft, and about 56. She had had no food for twenty-four hours, The summer was a very hot one, and she lived in a confined attic room. She was therefore in a very exhausted condition, and her muscular condition was far under par. I encouraged her to make a breakfast of bread, eggs, and tea. On calling about an hour after, her condition was unchanged, and, thinking that she only now required some oxytoxic agent to rouse

uterine action, I administered a drachm of tinctura ergotæ. This was several times repeated during the forenoon without any definite result. I was unwilling to leave, as the condition of the parts led me to expect a very speedy termination to the labour, should even slight uterine action come on. Beef-tea, wine, spirits, and strong infusion of tea were administered with evident relish, but also to no purpose. Digital irritation, even pinching the os, and external friction gave no better result. Fearing post-partum hemorrhage with such a condition of things, I was unwilling to use the long forceps. Every ordinary means of rousing uterine action I knew of I tried, but each and all failed to fulfil my expectations, and as other engagements were pressing, I resolved to try galvanism. Accordingly I sent for a galvanic machine, the appearance of which produced not a little amusement to the onlookers, although the patient eyed it with great apprehension. The first application was continued for two minutes. The uterus slowly responded, and after some seconds a moderate pain, the first for eleven hours, was induced, and continued to increase until the current was interrupted. The head descended to the brim. I now waited about ten minutes, and again applied the current for three minutes, which had the effect of bringing the head into the brim. After an intermission of five minutes, at the urgent request of the patient, who, to my astonishment, was pleased with the sensation produced, I again applied it, and in two minutes the head was rapidly born. By manual traction the birth of a healthy male child was completed in exactly twenty-three minutes from the commencement of the application. I now directed the patient to keep her hands firmly pressed against the uterine tumour, and in ten minutes more I found the placenta in the vagina. Convalescence went on uninterruptedly.

The success of this case, and the evident pleasure with which its application was enjoyed by the patient, made me resolve to add galvanism to my list of agents for expediting protracted labours, especially for those specifically known as cases of *inertia uteri*. And as I experienced some difficulty in getting the female attendant to work the machine steadily, I procured one of Maw and Son's self-acting electro-magnetic machines, with its complement of appliances or electrodes.

In the spring of the following year, 1869, other two cases occurred, where similar conditions presented themselves.

Case 2.—Mrs. M., aged 35, a tall, well-made woman, the wife of a farm labourer, had parturient pains for about thirty hours. I saw her first on the afternoon of the second day. This was her ninth child; all the previous births were natural, the two last only being somewhat protracted, but were accom-

plished without instrumental aid. I found her very exhausted, and the pains had left her some hours. The os was well dilated, and the parts were quite flaccid. The head could just be felt a good way above the brim. For several hours I tried the usual means of rousing uterine action, but failed to elicit any response. The patient was very lethargic and disposed to sleep. But as the case seemed so simple, I was unwilling to let it become further protracted, and sent for my battery. Meanwhile I freely administered Liebig's extract of meat, spirits, ergot, and friction, but failed to rouse action. The first application of the current lasted ninety seconds; powerful uterine action was induced almost immediately, and the head came through the brim. The current was now interrupted, in imitation of natural pains, and uterine action ceased, but commenced again spontaneously at the end of two minutes. This spontaneous action, however, was very feeble, and at the request of the patient the current was again applied. The action now became so very powerful that the head was born in a very few seconds, and the birth of a female child was completed without further aid, the placenta following in about the usual time. In this case the after-pains, probably induced by the large amount of ergot administered, were so severe that they had to be allayed with opiates. She made a good recovery.

Case 3.—Mrs. M'D., 32, about 5 feet 8 inches in height, slender but well-made, was in labour with her seventh child. The labour had commenced about midnight, and I saw her early next morning. Uterine action was weak, and occurring only at irregular and distant intervals. The os was about the size of a shilling, but dilatable. There was no great mechanical obstruction, though the coccyx was unyielding and more than usually turned in, but the pelvis was roomy. I had attended her in three previous confinements, which were natural, and required only digital assistance. I advised a good breakfast, which was somewhat reluctantly taken. In the course of the afternoon I returned, and found that there had been no action and no alteration in the condition of parts since the morning visit. I administered a large dose of ergot, and left, with orders to give another in the course of half an hour. On calling at the end of the hour, I found no change. Beef-tea, spirits, strong infusion of tea and coffee, porter, ergot, walking, digital irritation, and external friction were employed from time to time, and several times during the day she slept for half an hour. Evening arrived, but still no action could be elicited, nor did the os further dilate. I now sent for the battery. The first application lasted for one minute. Slight pains were induced, but the os was inclined to contract rather

than dilate. I interrupted the current for five minutes, and kept up digital dilatation of the os; meanwhile, during the next several applications, I kept my fingers in the os, so as to maintain dilatation and allow the head the more easily and with less obstruction to descend. The patient at first was very apprehensive of danger, and, thinking that there was no hope of her recovery, took a very affecting farewell of her husband, whose distress of mind during the operation was very audible from the next room. The patient now requested the application of the current more frequently than I considered advisable, and actually sent a message to her distracted husband "that she was enjoying it very much, and felt it doing her good." The descent of the head, however, was slower than I had experienced in the two previous cases. The tip of the coccyx offered some slight obstruction, but I persevered in the application, and in 35 minutes the head was born, and the birth of a large male child completed by manual traction. The placenta had to be removed, and there was a slight tendency to hemorrhage, which ceased, however, on the administration of spirits and the application of the binder. She made a slow recovery, which I accounted for by the exhausted state of her system, her family having followed each other at about 14 months' interval. This woman always recovered fast after her confinements, thus showing that she had good recuperative powers.

These cases, though few in number, are sufficient to convince me that for some cases of labour we have in galvanism a potent power for good. That the results I obtained were due to galvanism I have not the least doubt. That the force evolved by the uterus was galvanic I doubt, but that galvanism roused and kept up a power inherent in the uterus, I think cannot be denied by the most sceptical. Ramsbotham, of London, in 1834, used galvanism, and was favourably impressed with its use in obstetrics and gynæcology. Five years later, Killian invented a galvanic forceps, and after six years' use gave it up, the results having fallen short of his expectations. In 1844, Hoeninger and Jacobi used it for the induction of premature labour. To Radford, of Manchester, however, belongs the merit of first making a practical acquaintance with its use in obstetrics, and he became favourably impressed with its usefulness. The late Sir J. Y. Simpson tried it in eight cases, but considered it useless as an oxytoxic agent, and of no utility in parturition. The method of application used by Radford and Sir J. Y. Simpson seems to have been suggested by Killian's galvanic forceps, viz., that of direct application of the electrode to the os and vaginal walls. Sir James held the opinion that if the galvanic machine had any oxytoxic influence at all, it

was not by the galvanism *per se*, but by the irritating influence of the metallic instrument used—the catalytic action, if I might so use the term. He held the opinion advanced by Jordan that the uterus, like the dartos in the male, being of similar structure, is incapable of being excited to contraction by the agency of galvanism. That the presence of the instrument in the vagina had some influence on the action of the uterus I do not venture here to deny, but that galvanism has an influence on the action of the uterus in intensifying its expulsive power, either directly or indirectly, I think the cases recorded above go far to prove; and the more recent experiments of Oser and Schlesinger stampthe assertion with authority, for they have demonstrated that electrical excitation of the central end of a spinal nerve calls forth, in from five to fifteen seconds, energetic contractions of the uterus

The method of application I have adopted still further proves that contractions of the uterus occur by the use of galvanism without introducing any instrument into the vagina. I might ask, if the contractions are caused by the presence in the vagina of the metallic electrode only, why make use of galvanism at all? Why not treat every case of *inertia uteri* by inserting into the vagina a piece of metal? If the catalytic theory be correct, then this treatment should be legitimate and successful. I am not aware that the theory has had any practical outcome.

My method of application is as follows:—One of the electrodes, a flat piece of metal the size of two hands opened, and joined in the line of their long axis, and curved to fit the transverse abdominal curve, is applied to the abdominal parietes and retained by the patient's hands, a layer of blanket being between the metal and the fingers to prevent the current entering by the fingers. The other pole is placed against the perineum, and retained by folds of blanket or a small pillow. The force can be moderated or increased at will, by the left hand of the accoucheur working the "regulator," while the right hand manipulates the child's head. The electrodes are now attached to the machine, and the circle being thus completed, the current is set on, and in obedience to a law in electro-physics, takes the shortest and most direct route from the abdominal to the perineal electrode, or *vice versa*. The uterus lying in this direct line shares in the influence; and in obedience to another law, its fibres contract, and in so doing aid in expelling the uterine contents, its action being limited by the electric force evolved, the quantity and condition of the muscular fibre of the uterus, and the calibre and patency of the passages. In this way the contracting or expelling force acts in the direction of the line of axis of the uterus. The

induced contraction of the abdominal muscles must also exercise a very considerable influence over the expulsive process, this contraction lessening the size of the abdominal cavity and forcing outwards its contents. When the head descends and comes towards the perineum I shift the perineal electrode on to the coccygeal region, thus exercising a *vis a tergo*, and relieving the perineal muscles of their tonic contractions, which now become flaccid and permit dilatation. The head is thus born in the proper line of the axis of the outlet, with freedom. Possibly the action might be as powerful if the electrodes were placed anywhere on the abdominal parietes; but I prefer to place the one in front and the other over the coccyx, so that the action may be directed as nearly as possible in the long axis of the uterus and the outlet. If one of the poles of the battery were applied to the abdominal parietes, and the other to the os uteri, the whole uterus would be thrown into tonic contractions, the os would consequently be narrowed, and labour obstructed. In the cases recorded above, however, the current was not of sufficient strength to penetrate the whole body of the uterus, except in Case 3, where the os contracted slightly.

The electrodes I use for obstetrical purposes are two in number—the abdominal and the perineal. The abdominal is formed of two separate blades, each 6 inches long, 2 inches broad, and about $\frac{1}{8}$th inch in thickness, slightly curved, transversely and longitudinally, to fit the curve of the enlarged abdomen, and joined together by a hinge, which permits of their being separated for facility of carrying. The perineal is a solid or hollow ovoid, 4 inches in longitudinal diameter, $1\frac{3}{4}$ inches transverse, and $\frac{1}{2}$ inch in thickness. Instead of being a hollow ovoid, it may, for lightness sake, be of a scaphoidal form.

If the waters have escaped too soon, the parts should be well lubricated, to facilitate the descent of the head. In cases of *inertia uteri* the child will usually be born in a very few minutes after the application has commenced. But should a longer time be required, then the application must be intermitted, so as far as possible to imitate the natural pains. Were it otherwise, the same consequences might occur as sometimes happens when ergot has been indiscreetly administered, viz., too violent and continuous action, with the risk of rupture or stoppage of the placental circulation and the death of the child.

Regarding the use of this agent, galvanism should be used when the os is dilated or dilatable; when the uterine action has ceased, and cannot be roused by stimulants, friction, or other oxytoxic agents; when there is evidence of uterine action becoming weak, irregular, or intermittent—time will

will thus be saved; when there is post-partum flaccidity of the uterus, or post-partum hemorrhage, not caused by hour-glass contractions. In the latter case, I should first administer a 15 or 20 gr. dose of chloral (an excellent co-ordinator (?) of nerve force), wait for some minutes, then apply the current. In short, it is necessary and useful when there is simple, slight, additional expulsive force required, that is, in cases of *inertia uteri*, with a well-formed pelvis.

It might also be used for the expulsion of clots or air, conditions very troublesome, and often giving rise to grave symptoms in stout women during post-partum convalescence. It might be used with advantage in cases of uterine hemorrhage caused by the pressure of fibrous or other tumours, or in their expulsion. Were an instrument made to encircle the tumour, and the current applied sufficiently long to stop the circulation, coagulation might take place in the vessels, and the tumour might die for want of nourishment. Ergot could not be given in doses sufficiently large to accomplish this without grave risk, but galvanism can be localized. It is also free from the risks attendant on the knife. Of course, these later remarks are entirely speculative, and my sanguine hopes of its practical application in these cases may be discouraged by the opinions of men of more extended experience; but my success with this agent in these cases of *inertia uteri* has emboldened me thus far to trace its probable application.

I would recommend galvanism, therefore, in cases of sluggish uterine action, for its use would save the time of the accoucheur, and shorten the labour of the mother. Its use in the latter case cannot be over-estimated, when we remember the frequent baneful effects of protracted labours. Its use at the time, and by the method I have indicated, is not attended by any danger to the mother or the child. It is pleasant to the mother, and hope is revived within her when she feels that its application is doing good. They all expressed themselves agreeably surprised.—*Edin. Medical Journal, Sept.* 1873, *p.* 243.

89.—ON THE TREATMENT OF PUERPERAL CONVULSIONS.

By Dr. ROBERT BARNES.

[The following is an abstract of that part of Dr. Barnes's Lumleian lectures which relates to the treatment of convulsive diseases.]

Upon the treatment of convulsive diseases I have nothing new to offer in the way of remedies, or even of methods of applying them. The utmost I pretend to do is, to strengthen the rational basis, the principles, upon which the treatment

should be conducted; and to show why certain agents, still of conventional reputation, should be rejected.

The principles of treatment flow logically from the view we have taken of the etiology and nature of the diseases we have to deal with. Four cardinal principles may be laid down for our guidance:—

1. To moderate central nervous irritability.
2. To cut off emotional irritants or excitants.
3. To cut off peripheral irritants or excitants.
4. To eliminate all complicating morbid conditions.

To carry out all, or as many of these indications as we can, should be our aim in the management of all convulsive diseases. But the varying circumstances of different diseases and of different cases will prompt us to vary the order in which these indications shall be taken. For example, in all the convulsions of pregnancy the question surges up, Shall we try to subdue the central nervous irritability by putting an end to the pregnancy which produced it? The affirmative presents itself with cogent force. Yet it is not always possible or wise to resort to this measure, at least in the first instance. The indication is most clear when eclampsia breaks out after the sixth month of gestation. Here we have not only intense exaltation of nervous irritability, but an active blood-poison constantly keeping up this irritability, and which we know from experience will rarely disappear until the pregnancy which produced it is brought to an end. Often, indeed, the disease itself will determine the labour. In this case we have but to follow the lead, and accelerate it judiciously. The urgency is pressing; every fit is a source of fresh danger to the brain. Prompt action, by averting one fit, may save life. Still we may err by precipitation. If we hurry on the labour too quickly, if, in short, we carry out what the French call the *accouchement forcé*, we may do irreparable mischief. The violence of the necessary manœuvres will be a source of so much irritation as to add to the severity of the fits, and to add the depressing influence of shock. There cannot, I am convinced by observation even more than by theory, be a more fatal error than to follow the precept still inculcated and practised by many, to deliver as quickly as possible. The simplest measure which involves the least possible irritation is the best. That is, to puncture the membranes and leave the rest to Nature, at least until we see she fails to carry on the process. And since even the gentlest examination is often enough to provoke a fit, I would advise the previous induction of anæsthesia by chloroform. Under the cover of this state, the catheter should first be passed to secure an empty bladder, and to procure a good specimen of urine for testing. Then, at the same sitting, the

membranes should be punctured by a quill, stilet, or other suitable instrument. The diminution of the volume of the uterus by the draining off of the liquor amnii, lessening the pressure upon the vessels and the vascular tension, gives sensible relief. But another good effect generally follows: one it is true not without occasional drawbacks, but still a risk that must be encountered. The good effect is this; the moment labour is started a call is made upon the nervous centres for nerve-force to be expended upon the uterus. It is, indeed, a matter of observation that uterine action will often excite a convulsion. But, upon the whole, I am disposed to think that it acts beneficially; and we shall be the less afraid of calling it into operation if we reflect—first, that labour must take place, and that it cannot be effected without this uterine action; and, secondly, that we can greatly diminish the excess of irritability by the use of chloroform.

The expediency of inducing labour when there is albuminuria without convulsion is more doubtful. As we have seen, it is not certain that convulsions will break out.

The induction of labour is the means of carrying off, or of discharging, the excess of nervous tension. But something more is commonly required. We cannot complete labour all at once. Sometimes we must wait, and, not seldom, when the nerve-force has once got into a wrong channel we shall fail to turn it all to the proper direction. Measures for moderating the excess of central irritability are almost always useful. The most available of these is the induction of anæsthesia by chloroform. It should be carried to the surgical degree—that is, to the extent of rendering the spinal cord irresponsive to irritation of the sphincters. The beneficial effect of this is sometimes very striking. The convulsive fit is rendered less violent; it is shortened. When anæsthesia is induced in anticipation of a fit, this may be almost entirely averted, and is sure to be moderated. The evidence in favour of chloroform has been greatly accumulating since Simpson's time. Chloroform blots out memory, one source of emotion; it cuts off perception, another source of emotion; it lessens reflex irritability. When chloroform or equivalent anæsthetics cannot be given, we must act on the same indications. We must procure absolute rest; exclude noise, light; avoid all suggestion of disagreeable ideas; avoid all irritation of the skin. All this is especially necessary at the acme of spinal and cerebral irritability when a fit is on or impending. But an essential condition for obtaining from chloroform the full benefit it is capable of giving, is to keep close watch over the patient, so as to be ready to administer it on the slightest warning of a fit.

How it is that chloroform acts in averting or shortening a

fit may perhaps be explained by the following observation of Achille Foville. The cessation of an attack is the consequence of the asphyxia which itself produced. The quicker the asphyxia, the more quickly is its action felt upon the cord, rendering it incapable of reacting. Thus the danger is averted by its very excess. Chloroform, by inducing asphyxia, acts in a similar way.

But whatever the explanation, the use of chloroform now rests upon a solid foundation of clinical facts. When a fit overtakes a patient, the action of chloroform is too slow. It is therefore my intention on the next occasion to administer the nitrite of amyl.

In fulfilment of the same indication we may derive great assistance from opium, belladonna, and bromide of potassium.

When the patient can swallow, in the intervals of the fits, the bromide of potassium or of ammonium may be given in scruple or half-drachm doses every three or four hours. But in the first instance it is more useful to give a scruple or half-drachm dose of chloral. This, like chloroform, removes all emotional sources of irritation, and lessens the sensitiveness of the nervous centres to peripheral irritation. The sleep it procures is eminently beneficial. I entirely concur in the praises which many practitioners have bestowed upon this most precious remedy in eclampsia.

We cannot yet discuss the treatment of eclampsia without referring to the practice of bleeding. To advocate anæsthesia is practically to condemn venesection. Not that there is any necessary antagonism, but that, as a matter of fact or of fashion, chloroform is superseding venesection. I am one of those who think there is more of fashion than of wisdom in the almost absolute oblivion of the lancet. But in this particular case I do not regret the disuse into which it is falling. It is very easy to tell of cases in which bleeding has been followed by recovery, and of other cases in which other treatment has been followed by death. I believe I have seen distinct relief ensue upon moderate abstraction of blood from the arm or by the application of leeches to the temples. And where there is distinct evidence of plethora with marked engorgement of the vessels of the face, it is judicious, I think, to apply eight or twelve leeches to the temples, but not to the exclusion of anæsthesia. In delicate women with a feeble circulation bleeding in any form should be rigorously condemned. And we must not forget that the process of labour is usually attended by a loss of blood quite as great as is good for the patient.

In climacteric epilepsy the abstraction of a moderate quantity of blood by leeches to the temples, or by cupping at the

back of the neck, just before the expected return of the attack, is often eminently useful.

The second indication, to cut off emotional excitants, is one that is generally studied.

The third indication, that to cut off physical peripheral excitants, demands more attention. The first impulse of many when they see a person in a fit is to dash cold water in the face. In eclampsia it is decidedly injurious. I have seen it provoke a fit. Another not uncommon error in eclampsia is to apply blisters to the nucha, or mustard poultices to the calves. These applications do exactly what ought not to be done. The irritation they produce when the whole surface is in a state of hyperesthesia, is doubly prejudicial. There is no fact in medicine of which a stronger conviction has been forced upon me by observation than this, that all peripheral irritation is injurious in eclampsia. It is a sin against physiology. Hence the rule, when the situation dictates manipulation of any kind, to lull the system in the artificial sleep of anæsthesia before passing the catheter, before making a uterine examination, or proceeding to induce labour.

These principles of action apply with almost equal cogency to the treatment of epilepsy in the pregnant state; they apply with quite equal cogency to the treatment of obstinate vomiting in pregnancy. When vomiting, in severe and uncontrollable degree, sets in about the third month, the question as to inducing labour presents itself with peculiar anxiety. On the one hand, if it be resorted to at once, there may be room for the misgiving that a grave step has been taken unnecessarily. On the other hand, the fatal progress is apt to steal on insidiously but rapidly, so that the time for hopeful action may quickly pass away. If the pulse have risen to 120 or 130, if it be small, indicating prostration; if there be marked Hippocratic countenance, considerable emaciation, continuous difficulty in keeping down food, sleeplessness, and especially any degree of delirium, it is highly probable that the induction of abortion will be too late. It may even provoke distress which will accelerate the fatal issue. The same observations apply to obstinate vomiting in the latter months. The indication to interpose early is all the stronger if there be albuminuria; for the concomitant blood-disease will almost infallibly keep up the irritability of the nervous centres and the vomiting. But in several fatal cases which I have seen there was no albumen in the urine, so that, whatever poison there were in the blood, the condition was probably different from that which we call uremia. Comparing these cases with the phenomena of acute atrophy of the liver, and with other cases fo rapid sinking in pregnancy, I cannot help suspecting that

there is developed some graver systemic or organic disorder than has been yet recognised. In some cases I have no doubt the delirium witnessed towards the close, the irritative fever, are the result mainly of starvation.

It is wiser then to err on the side of safety to the mother, and rather to induce labour too soon than to temporise until it is too late. Unless we can, within a short time, get wholesome nutriment into the system, the system will feed upon itself; and the nervous centres, partaking of the general exhaustion, may soon be paralysed.

To counteract this progressive starvation is one of the greatest practical difficulties. Enemata of beef-tea containing brandy or port wine, and sometimes half a drachm of chloral, are often of the greatest benefit. But in cases of extreme anxiety there is another remedy which has not yet been sufficiently, if at all tried, and which is full of promise. I mean the transfusion of blood. One successful case has been recorded of transfusion in exhaustion from puerperal convulsions.

In the case of exhaustion from obstinate vomiting, if used betimes, the prospect is better still, unless, indeed, the patient be struck with that deadly disease at the probable existence of which I have hinted.

In the chronic state of the menstrual or climacteric epilepsy, bromide of potassium, belladonna, and various metallic preparations, and a carefully ordered hygiene, are our chief allies.

I have not yet fairly tried Voisin's plan of giving the bromide. But it appears to me eminently deserving of being rigorously tested. It consists, as described in his admirable article on Epilepsy in the Nouveau Dictionnaire de Médecine, in giving from two to twelve grammes daily a few minutes before meals, and in persisting unintermittingly for a year or more, testing the action of the bromide by applying a spoon to the epiglottis. When reflex nausea is no longer excited by this test, we know that the drug is acting on the rachidian bulb diminishing its excito-motor force. As Voisin truly says, "in a chronic disease we must have a chronic medication. Bromide of potassium must be an aliment." Diuretics must be given with it; and occasionally iron to obviate anemia.

Trousseau's plan of giving belladonna continuously for one, two, or more years, is another way of carrying out the same indication. This, he says, is more especially useful in the epileptic vertigo. I am inclined to suspect that the disappment at times experienced with these remedies is in some measure due to the want of perseverance in keeping up the full doses over a long space of time, and in many cases—my remark applies especially to women—to the neglect to treat the frequently attending ovario-uterine complications.

The value of the metals in the treatment of convulsive diseases, has always been recognised. It would be impertinence on my part to dwell upon the subject. I would simply ask leave to mention that many years ago I proposed, and extensively tried, the combination of zinc with phosphoric acid. The state of the urine should be observed; and, in any case, the secretions should be carefully regulated by alkaline salines, aloetic and mercurial pills, or Püllna or Friedrichshall waters. To meet the fit itself, chloroform should be kept at hand, and a bit of india-rubber to slip between the teeth.

The indication to cut off all complicating morbid conditions is one that rarely admits of being fulfilled in the urgent cases of eclampsia and of the vomiting of pregnancy. Purgatives, and those not the gentlest, are commonly given in eclampsia, the motive being to remove any possible irritating matters from the intestinal canal. It is a routine practice, which is occasionally useful, as in those cases where a fit has come on soon after a heavy meal. An emetic would act better still. But in the majority of cases I do not think I have seen any good from the practice. Some violence is often done in forcing open the jaws to place calomel or croton oil on the tongue, and this is bad.

The elimination of all complicating morbid conditions is especially necessary in the prevention and treatment of the convulsive and other nervous diseases of non-pregnant women. In a large proportion of cases, indeed, this constitutes the greater part of the treatment. This applies particularly to neuralgia, to hysteria, and, in a lesser degree, to the epilepsy of the climacteric period.

The particular form of dysmenorrhea which is most commonly the attendant and forerunner of nervous disorder is that which depends upon obstruction or partial retention of the menstrual fluid. I may state that the following sequence of facts is established by an overwhelming mass of clinical observations.

1. Beginning with the menstrual function, there is the pain which is one of the features of dysmenorrhea.

2. There is gradual wear and tear of the nervous system, attended by degradation of the blood.

3. Increased susceptibility to physical and mental impressions.

4. Where marriage ensues, it is generally unfruitful; proving again, by another test, that there exists an impediment to the due performance of the ovario-uterine functions.

5. Where the obstruction is removed, as in most cases it can be, we find the preceding conditions gradually disappear.

If, on the other hand, the dysmenorrhea be cured early—

that is, before hysteria, neuralgia, or epilepsy have shown themselves,—these disorders will, in high probability, not appear at all.

In single women dysmenorrhea is the most frequent attendant or exciting cause of hysteria or epilepsy. In a certain proportion of cases, however, no abnormality of structure or function of the ovaries or uterus is apparent. The exciting cause may spring up elsewhere; but in all there is a special proclivity developed by the normal ovario-uterine stimulus.

In married women, and in those who have borne children, not only may dysmenorrhea arise, but metritis, congestion, displacement, and other affections are more frequent. They hardly ever fail to induce that general debility and nervous prostration which predispose to nervous disorders. It is generally a hopeless task to cure these nervous disorders, unless we begin by relieving the local disorders upon which they so greatly depend.

Before closing, I must beg leave to retrace my steps rapidly, and to sum up in a few propositions the principal points of my theme.

1. Pregnancy and labour require for their due fulfilment an extraordinary supply of nerve-force.

2. This extraordinary supply of nerve-force implies a corresponding organic development of the spinal cord.

3. The provision of an extraordinary supply of nerve-force implies a greatly augmented irritability of the nervous centres, rendering them more susceptible to emotional and peripheral impressions.

4. The disturbances in nutrition occasioned by pregnancy almost always entail some alteration of the blood, which increases the irritability of the nervous centres, and favours the evocation of any latent convulsive or rather nervous diathesis, as chorea, epilepsy, or vomiting.

5. When the blood-change wrought by pregnancy is marked by albuminuria, a poisonous action of peculiar intensity is exerted upon the nervous centres tending to produce eclampsia.

6. Obstinate vomiting in pregnancy probably sometimes proves fatal by the development of an unknown organic or systemic morbid process.

7. Menstruation resembles pregnancy in giving rise to an exalted central nervous erethism, and ovulation is a primary exciting cause of epileptic, vomitive, and hysterical convulsion.

8. At the climacteric age, again, there is renewed susceptibility to convulsive disease.

9. Pregnancy, by evoking or producing convulsive diseases, under certain known and passing conditions, put to the test the various theories of the pathogeny of these diseases.

10. The rational treatment of convulsive diseases in women must take into account the two great factors in the production of these diseases—namely, exalted nervous irritability under the stimulus of the reproductive function, and lowered or empoisoned conditions of the blood.

If it be objected that the views I have ventured to lay before you seem to be false in colour and form, wanting in breadth and perspective, I might reply that to deny this absolutely would be to arrogate to oneself freedom from human infirmity. The mind, like a mirror, can only reflect the impressions it receives. Our care must be to keep the mirror bright and even, so that it may reflect truly. This I have endeavoured to do. With what success must be determined by comparing my reflections with those coming from mirrors that are brighter. One thing only do I ask. It is that these mirrors may be so set as to take in the objects which have been reflected from mine.—*Obstetrical Journal, July* 1873, *p.* 267.

90.—ENEMATA OF CHLORAL HYDRATE IN PUERPERAL CONVULSIONS.

By Dr. JOHN BARCLAY, Leicester.

The notice in a contemporary of a somewhat similar case, induces me to mention an instance of the successful administration of chloral in a most severe case that I recently saw in conjunction with my friend Dr. Hulme, of Wigston Magna.

Mrs. T., aged 22, primipara, who had had slight anasarca before her confinement, was delivered at 5 p.m. on February 9th. The labour was satisfactory, the placenta following naturally. Shortly afterwards convulsions set in, and continued almost without intermission. I saw her first at 2 p.m. on February 10th, the convulsions having lasted twenty hours. She was totally insensible, cold, and clammy; the pupils were largely dilated; pulse 180. She was unable to swallow; the fæces and urine passed unconsciously; she had had turpentine enemata with no benefit. While I was there she had the most horrible set of convulsions I ever witnessed. In fact, I thought she was dead, respiration ceased for so long. Unable to give any remedy by the mouth, I suggested, as a forlorn hope, half a drachm of chloral in a small starch enema, to be repeated once. I saw her again at 6 p.m., and found her in a quiet natural slumber that I did not disturb. She had fallen into it, and had had no more convulsions after the second enema.

Passing through the village next day, the 11th, I found her only a little stupid; the pupils were natural; pulse 108; the milk and lochia appearing. She made a good recovery, and is now well and nursing her baby.—*British Medical Journal, May* 31, 1873, *p.* 611.

91.—CASE OF PUERPERAL CONVULSIONS AFTER DELIVERY.

By Dr. ALLAN D. MACKAY, of Stony Stratford.

[The patient, who had been attended by a midwife in the morning, had had three convulsions before being seen by Dr. Mackay, in the evening, which were described as "slight, bad, and worse" by the friends. There had been anasarca of the lower limbs, and various head symptoms indicative of uræmic poisoning.]

I drew off more than a pint of urine with the catheter, and found it of specific gravity 1018, and containing much albumen. I prescribed perfect quietude, broth and milk for her diet, and a draught containing 25 grains of hydrate of chloral to be given as soon as she could swallow, in case another convulsion should come on; if none recurred, that she should be left alone, as far as medicine was concerned, till I saw her the next day.

On the following day I found her with a tongue loaded with a creamy fur, very swollen, and showing traces of having been bitten. Her pulse was 100. She had headache, but could "see a little." No urine had been passed, so I again drew it off with the catheter, and it was still albuminous. The legs still pitted a little. But now for the practical part of my paper, and it is solely for this that I bring the matter before you to-day. Soon after I left her on the previous evening, about 7 p.m., she had "a severe convulsion." There had not been then time for the medicine to reach her. At 11 p.m. she had "a still more severe convulsion;" when this ceased, she was able to swallow the draught (25 grains), and was soon afterwards asleep. She slept for four hours, then woke, saying she "could see a little," and slept again and again. At night the chloral was repeated, and she had an aperient dose to take during the day.

On the 10th the tongue was cleaning; pulse 80. The urine was passed last night and this morning. She could see, though still "a little misty." She had to-day no headache, and there had been no more convulsions.

On the 11th the milk had come, and the urine was passed well. I need not detain you with the progress of the case. She got better each day, and is now perfectly well, the albumen quickly passing away from the urine.

In chloral, then, it would appear we have a grand assistant in these cases. For some time I doubted with myself (hating to believe too soon that the *post hoc* is *propter hoc*) whether the cessation of the convulsions could be due to the chloral or not;

but, considering that they were increasing in severity, it seemed that such might be the case.—*British Medical Journal, June* 21, 1873, *p.* 701.

92.—THE ANTICIPATION OF POST-PARTUM HEMORRHAGE.

By Dr. EWING WHITTLE, Lecturer on Medical Jurisprudence in the Royal Infirmary School of Medicine, Liverpool.

[More than twenty years ago Dr. Whittle read a paper before the Medical Society of Liverpool, showing that the occurrence of post-partum hemorrhage can be diagnosed beforehand, and being diagnosed can be prevented.]

Very early in my career in the practice of obstetric medicine, I observed that cases of parturition in which post-partum hemorrhage occurred were always marked by the pains during labour being of a peculiar character. Having my attention directed to this point, I further observed that post-partum hemorrhage *never* occurred in any case of labour in which this peculiar character of the pains was absent. The pains are of this kind: they are strong and quick; they do not gradually culminate into a strong pain and subside again, but they are sharp, quick, and cease almost suddenly; and the intervals between the pains are long in proportion to the length of the pains. In an ordinary case, for one or two hours before the completion of labour, the intervals will average about three times the length of the pains; *i.e.*, if the pains last each from fifty to sixty seconds, the intervals will average a little less than three minutes. Now, if the pains last each only from forty to fifty seconds, and are of the sharp character I have described, with intervals lasting five or six minutes, though the labour may proceed steadily, and the head advance a little with every pain, you will be sure to have hemorrhage after delivery is completed, unless you anticipate it by altering the character of the pains, in making the pains longer and the intervals shorter. It is very easy to understand how this comes to be the case; the uterus is contracting sharply, and then becoming fully relaxed; after the child is born, a relaxation follows; one or two sharp pains expel the placenta with a gush of blood, and the uterus again relaxes, continuing the same tendency which existed before the delivery of the child.

Some of my friends will say that this may be very well, but that all this may be prevented by carefully attending to the maintenance of a steady contraction of the uterus by following down the expulsion of the child and placenta, and keeping the uterus firmly grasped until all danger of relaxation has passed away; that, by attending to this point carefully, during an experience of many years, they have never had a serious case

of hemorrhage. But I would warn them that, no matter how long, through good fortune or good care, they may have escaped this painful trial, they never know when they may happen to fall in with a patient of weak, flabby fibre, and low nervous power, in whom they may find it impossible almost to induce active uterine contraction in time to save her from the effects of the hemorrhage.

Having recognised the necessity for interference in any case as soon as the os is fully dilated, I give a full dose of ergot. If this do not improve the character of the pains at the end of an hour, or thereabouts, I repeat it; I scarcely ever find a second dose necessary. In dealing with primiparæ, caution is required, first, not to administer ergot until the soft parts are pretty well dilated as well as the os uteri; and the drug should be administered, in much smaller doses, as it sometimes acts with unusual energy in primiparæ. Generally, in about twenty minutes or half an hour after the ergot has been administered, the pains increase in length and frequency, and when the labour is over, the uterus maintains a good contraction. The ergot which I use is a liquid extract twice the strength of that of the *Pharmacopœia*, of which I give a teaspoonful when I think a full dose is indicated.

I have pursued this practice now for more than twenty years. During this time I have attended 3750 labours, and among them I have had one case of *post partum* hemorrhage; that case occurred about three o'clock one winter's morning, when I happened to have no ergot with me. I foresaw that it would happen, and was able to prevent its being very serious by adopting the ordinary means.—*British Medical Journal, Sept.* 27, 1873, *p.* 370.

93.—ON THE INJECTION OF PERCHLORIDE OF IRON IN POST-PARTUM HEMORRHAGE.

By Dr. W. S. PLAYFAIR, Professor of Obstetric Medicine in King's College, and Examiner in Midwifery at the Royal College of Physicians, London.

[In a discussion which recently took place at the Obstetrical Society of London, upon the subject which forms the title of this paper, Dr. Playfair stated that he had in no case seen any injurious effects from the injection of a solution of perchloride of iron into the uterus, although admitting that an agent so potent should not be carelessly and indiscriminately used.]

By a somewhat curious coincidence a few days after the debate I had a case under my care in which I used it, and, as I firmly believe, saved by it the life of my patient. Yet very

grave and even alarming symptoms followed — due, it can hardly be doubted, to its employment, and I think that the case is sufficiently instructive to be worthy of record. It shows one class of dangers which may arise from it, and possibly the history will teach us how, under similar circumstances, these are to be avoided.

Two and a half years ago I saw, with Mr. Aikin, of Clifton Place, Sussex Square, a lady who was apparently at the point of death from post-partum hemorrhage. She had been confined of her fifth child rather more than two hours before I saw her, after a somewhat tedious labour, the breech presenting. All her other labours had been natural. She was a stout woman, thirty years of age. After delivery the uterus had contracted firmly, with no more discharge than usual. Mr. Aikin had stayed with her more than an hour, and had left her seemingly well and comfortable. Half an hour afterwards she had a tremendous gush of hemorrhage. Mr. Aikin was immediately summoned, and speedily arrived, accompanied by Mr. Rushforth, of Oxford Terrace. The patient was then collapsed and insensible, and to all appearance dead. Some brandy was introduced into the mouth through an aperture formed by the absence of one or two teeth, and a solution of perchloride of iron, which Mr. Aikin had fortunately with him, was at once injected into the uterus, and all further loss was checked. When I saw her shortly afterwards she was still collapsed and pulseless, and I immediately sent for the necessary apparatus for transfusion, which seemed to afford the only hope of saving her life. Before the instruments arrived, however, she had slightly rallied, and eventually made a good recovery, though she long remained blanched and anemic. Such was the formidable history of the patient previous to her present confinement.

On this occasion Mr. Aikin was unable to take charge of her, being confined to his home by illness, and I was asked to attend her in company with Mr. Rushforth. In no case is "forewarned, forearmed" a truer proverb than in relation to post-partum hemorrhage, and as we adopted every possible precaution to prevent it, we were in hopes that no repetition of the former flooding would occur. The head presented, and the labour was natural and easy. As the head descended a drachm of the liquid extract of ergot was administered. Firm pressure on the uterus was kept up as the child was expelled, and continued without intermission afterwards. A second dose of ergot was given shortly after delivery, immediately after the expulsion of the placenta. One or other of us kept kneading the uterus for three-quarters of an hour after the birth of the child. It contracted fairly, but not tightly, and it showed a

tendency to relax. Two or three times small pieces of ice were introduced into the uterus to promote contraction. All this time there was no unusual loss, and we considered any danger of hemorrhage to be over. Suddenly, and while the uterus was still grasped by the hand, an appalling flow of blood occurred. I immediately emptied the vagina of a mass of clots, and, as all means of promoting contraction had been already vigorously employed, I at once proceeded to inject a solution of the per-chloride of iron of the usual strength; and not a moment too soon, as the patient was already tossing about, sighing deeply, and showing the well-known formidable signs of collapse. As I injected I felt the uterus contracting round my hand, and not a drop more blood was lost. Nothing could be more rapid and satisfactory than the action of the remedy, and I honestly believe nothing else would have checked the flooding or enabled us to save the patient's life. For two days all went well. On the third day the pulse was 100, and the temperature 102°. The day following the pulse was 120, small and thready, the temperature 104° in the morning, and 105° in the evening, the tongue dry and black, and the general condition very alarming. There was no abdominal tenderness whatever. The uterus was somewhat large, reaching nearly to the level of the umbilicus. There was little or no discharge, and what there was was highly offensive. Eight ounces of brandy per diem were administered, and 30 minims of turpentine every sixth hour, and and a teaspoonful of Brande's beef jelly every hour. On internal examination, the whole vagina was found to be filled with small, hard, black clots, formed by the corrugating effects of the iron, and believing that the symptoms were probably due to the retention in utero and decomposition of similar clots, giving rise to septic absorption, the cavity of the uterus was freely washed out with Condy's fluid and water, by which several portions of broken-down coagula were removed. Next day things were worse rather than better, the temperature being 105¼°, pulse 130. There was some cough, with sibilant râles, over the right chest. Still there was no local tenderness or other symptoms. We then had the advantage of meeting Sir William Jenner in consultation. The general treatment was continued, the quantity of brandy being increased. With the view of reducing the hyperpyrexia gr. v. of sulphate of quinine in pill were administered every third hour. The intra-uterine injections of Condy were continued three times a day, and in the evening a large and highly offensive clot was ejected. Next morning the temperature had sunk to 102¼°, and the pulse to 100. Treatment as before. Quinine now given every fifth hour. In the evening the temperature had again risen to 104°. Another large coagulum was expelled

after injection. Next morning the temperature had fallen to 101¼, the pulse to 86, and all fetor had disappeared from the discharge. No more coagula were passed. It is needless to continue a record of the case, as the improvement from this date continued to be steady, and in a few days the patient was convalescent.

There can I think be little doubt as to the sequence of events which gave rise to these alarming symptoms. When the iron was injected, although the hand was in the uterus, and the clots within it had been as much as possible removed, blood was still pouring out abundantly. The powerful astringent at once corrugated all the blood and coagula it came in contact with, and these hardened clots filled up the uterus and the canal of the vagina. In due course these began to decompose, and septic absorption took place. By the finger and the intra-uterine injection they were gradually broken down and removed. The improvement unquestionably dated from the expulsion of the two large and decomposing coagula on the sixth and seventh days after delivery. Immediately after this happened, the temperature and pulse fell remarkably, and recovery commenced and continued uninterruptedly.

What then is the lesson to be learnt from this case? Is it that the risk is too great, and that the injection of the perchloride of iron should be banished from practice? I think most unquestionably not. I have little doubt, knowing what I did of the patient's former labour, and having already tried in vain all the anti-hemorrhagic treatment at our command, that without the perchloride the flooding would have proved fatal. It is indeed precisely in these inveterate cases, where every means of inducing uterine contraction proves unavailing, that it forms so invaluable a resource. Rather, I think, it should teach us to limit its use to these only—as, I believe, Dr. Barnes has all along taught. It shows also that the retention in utero of hardened coagula, liable to decomposition, may prove a source of danger hitherto unsuspected. With a knowledge of this fact it would be our duty to secure the expulsion of the coagula as soon as possible after all risk of hemorrhage had ceased, and make sure that there was a free exit for the discharge.

This would best be done by satisfying ourselves on the second or third day after delivery that the vagina is not filled with clots, and removing them if present, and by using antiseptic intra-uterine injections freely, as in the above case, should suspicious symptoms arise. With a knowledge of this source of danger, it might probably be avoided in most cases. Whether any other astringent fluid, such as the tincture of matico, the use of which was suggested at the Obstetrical

Society, would answer equally well in constringing the vessels from which the blood flows, and be less apt to produce hardened coagula, is well worthy of consideration. I question very much, however, if anything less than the most powerful and direct astringent is to be depended on.

Important as are the lessons this case has taught me, it has left me not a whit less a believer, but rather a firmer one, in this most invaluable remedy.—*Obstetrical Journal, May* 1873, *p.* 89.

94.—A CASE OF POST-PARTUM HEMORRHAGE TREATED BY THE INJECTION OF THE PERCHLORIDE OF IRON.

By Dr. THOMAS CHAMBERS, Physician to the Chelsea Hospital for Women, &c.

[Nearly every case of post-partum hemorrhage can be treated successfully by external pressure and cold, but there are cases which quite resist such treatment, and it is for these that the perchloride of iron is said to be so successfully employed.]

Mrs. P., aged 30, a thin, delicate-fibred, nervous woman; third pregnancy, full time; last pregnant five years ago. I saw this lady for the first time at 11 p.m., July 10. The pains were short, and recurring once in fifteen or twenty minutes. The os uteri being high up and not larger than a shilling, I prescribed twenty drops of opium in a little brandy-and-water, and returned home. At six o'clock on the following morning (July 11th) I was again sent for. From this time labour progressed quietly until 11.30, when a sudden and long-continued pain came on, which expelled the child. After the separation of the child, the right hand was placed upon the abdomen, when the uterus was found to be almost as large as before the expulsion of the child; it was very soft and flabby, having a doughy feel. When the hand was pressed upon it, it seemed to sink *into it* as if pressed into a soft body. As there was no external hemorrhage, and as the cord was soft and lax, it was clear the placenta was detached, and, in all probability, blocking up the os uteri, and that the enlarged uterus was filled with blood. When the placenta was withdrawn it was followed immediately by a large clot and most apalling hemorrhage. Free pressure was made outside by the right hand, and inside by the left, but notwithstanding every effort, the blood still flowed apace, streaming down my left arm, and running off the elbow on to the carpet. In a few minutes the patient became completely blanched, the pulse all but imperceptible, the respirations sighing and spasmodic, with general jactitation. Still the uterus remained soft and flabby, and

there was every prospect of an early death. Here was a case admitting of no delay by way of consultation or otherwise. I sent to the nearest chemist for tinc. ferri perchloridi ℥ vj. Half of this I directed the nurse to mix with a pint of water, still keeping up both external and internal pressure. When all was ready, I passed the long tube up the palm of my left hand to the fundus uteri. In two minutes from the first injection I could feel the uterus gradually contracting around my hand, and with this contraction the bleeding ceased, the coagula passing out of the uterus with the hand as the uterus contracted. In five minutes from the first injection the uterus had freely contracted, and all hemorrhage had ceased; and, for the first time, there seemed a chance for the patient's life. Nothing could exceed the rapidity of action of this remedy. The satisfaction felt at such success under circumstances so urgent requires to be experienced in order to be duly appreciated. I honestly believe that no other remedy with which we are at present acquainted could have rescued this poor lady from immediate death. The fact cannot be too often or too deeply impressed on the professional mind that the treatment of cases such as this must be *prompt* and effective, or the patient is lost.

It may be remarked that this was a capital case for ergot. Just so. I gave a dose of ergot in a little brandy before removing the placenta and immediately afterwards. It must not be forgotten that the ergot must be absorbed into the circulation before it can induce uterine contraction. Now, in acute hemorrhage, such as has been described above, one of the first and most important symptoms is the almost total loss of pulse. It must follow as a matter of course that when the circulation is reduced to a minimum the absorbing powers must be reduced in a like degree; this being the case, it will follow that ergot can be but of little if of any value in such cases. And what took place in this case will doubtless take place in others under similar circumstances—viz., the ergot and brandy remained in the stomach for a time, and then were ejected by vomiting. Very free vomiting took place in this case which complicated matters considerably.

The immediate contraction which followed the injection was very remarkable. The right hand was kept on the uterus for half an hour after the hemorrhage ceased, as a matter of precaution. The uterus could be distinctly felt to contract into a hardened body, and again relax at frequent intervals, but no bleeding accompanied or followed these contractions.

The patient vomited violently immediately after the injection, nothing being ejected but the brandy and ergot, which had been freely taken at the early stage of the case. As far

as one could judge, it was much in the same state as when taken into the stomach, indicating pretty clearly that little or no absorption had taken place. This lady made a rapid and satisfactory recovery without a single bad symptom. July 31st. She is to-day in her drawing-room, her only complaint being that she is much weaker this time than on former occasions. This is just the kind of case which Dr. Barnes has indicated as suitable for the perchloride of iron treatment—one which, in all human probability, would have been lost but for the timely aid afforded by this powerful remedy.—*Obstetrical Journal, Oct.* 1873, *p.* 446.

95.—THE INJECTION OF PERCHLORIDE OF IRON IN PUERPERAL HEMORRHAGE.

By Dr. A. B. STEELE, Physician to the Lying-in Hospital, Liverpool.

[It was only owing to the very strong opinion expressed by Dr. Barnes that the writer of this paper first resolved to put to the test of practical experience, the intra-uterine injection of perchloride of iron to arrest post-partum hemorrhage.]

As I am now in a position to speak from bedside observation, and finding from recent discussions that the treatment in question is still *sub judice*, I feel bound to contribute my quota to the settlement of so important a question in obstetric practice.

A circumstance which more immediately determined my adoption of this mode of treatment was the accident of my listening to a graphic description of a case by Dr. Williams of Wrexham, given at a meeting of the North Wales Branch of the British Medical Association, held last summer at Bala, at which I had the good fortune to be present. A lady who had been attended by Dr. Williams in several labours invariably suffered from post-partum hemorrhage to a degree which caused much anxiety for her immediate safety, and rendered each approaching confinement a source of dread to herself and her friends. On the occasion of her last confinement, Dr. Williams determined to try the effect of the iron injection as recommended by Dr. Barnes, and accordingly this was done immediately after the expulsion of the placenta and before hemorrhage had commenced. No sooner was the operation completed than the patient, notwithstanding her usual dread of impending hemorrhage, at once exclaimed, "I am better now, and I know I shall have no bleeding this time;" and such proved to be the case, and her recovery was excellent. This case impressed me so strongly that I resolved to adopt the plan on the first suitable

occasion, which shortly after presented itself in the following case :—

A patient of my own, nearly forty years of age, of tender, delicate frame, deficient muscular tone and energy, large dilated veins, and the subject of hemorrhoids, which in the latter months of pregnancy became so aggravated as to necessitate their removal by the clamp and cautery, was taken in labour for the second time. Her first confinement, a year previous, was protracted and difficult from uterine and general inertia, rendering forceps delivery necessary, extraction not being effected without long and forcible traction; hemorrhage ensued, which was controlled by the ordinary measures, but a subsequent draining of blood continued, which, although not excessive in quantity, was nevertheless a source of much anxiety in her already exhausted condition. She however ultimately recovered after a tedious and troublesome puerperal period. The child was born alive, but died in a few weeks from diarrhœa and atrophy consequent upon loss of breast milk and general debility.

The second labour was almost as difficult and protracted as the first; she was delivered after long traction with the forceps of a fine living female child. Hemorrhage again set in immediately after the completion of labour. I at once injected a solution of iron, one part of liq. ferri perchloridi fortior. to four of water, which in a few minutes completely controlled all bleeding, and caused firm general contraction of the uterus, contrasting favourably with the imperfect and unreliable contraction so common under these circumstances; and which on the former occasion rendered her condition critical for a considerable time. Her recovery, although complicated by constitutional delicacy and feebleness, was nevertheless much more favourable than in her first confinement, and the child lived and thrived well.

The following case, which occurred shortly after that just related, is even more specially illustrative of the value of the iron injection, not only in puerperal hemorrhage, but also in the hemorrhages of abortion.

A patient about twenty-five years of age, a fair, delicate-looking woman, the mother of one child, first consulted me for a constant, and at times profuse loss of blood, which had lasted for many weeks, after an abortion at the fourth month. I opened up the cervix with tents and swabbed the uterine cavity freely with the undiluted liq. ferri perch. fortior.; after two applications all bleeding ceased, and in a short time she was quite well. About a year later I was called to see her in consultation with her medical attendant, in consequence of post-

partum hemorrhage of a formidable character, which had come on about an hour after the completion of labour, and had already caused great depression, approaching to collapse. The bleeding was promptly checked by compression, cold cloths, and the other ordinary means, but reaction was slow in taking place; the patient remained for some hours in feeble, excited state, with delirium and other symptoms of constitutional disturbance, which required close watching for two or three days. She recovered slowly but completely. In about twelve months after this she was again taken in labour, and I saw her as soon as the pains set in. Her labour was easy and somewhat rapid. Every precaution in anticipation of flooding was adopted: a full dose of ergot just before the expulsion of the head, careful compression of uterus with the hand throughout and subsequent to the expulsive stage, compress and binder, and so on. For a short time after delivery all went on well, and I left the room, but was soon recalled by the nurse, as the patient told her "there was a good deal coming away." I at once recognised the effects of hemorrhage in her pallid lips and faint condition, and found a large quantity of coagula in the bed and in the vagina, from whence fluid blood was still flowing. The uterus, although not entirely flaccid, was doughy, and did not readily respond to compression. I hastily prepared a strong solution of the solid perchloride (which fortunately I had brought with me), and having cleared the uterus and vagina from clots, during which process I could feel the warm stream still flowing, I threw up about a quart of the fluid, which at once checked the bleeding, and in a few mintes the uterus, and especially the os, was firmly contracted. No further bleeding nor any untoward symptoms followed, and the patient made a better and quicker recovery than she had ever done on former occasions. In each of these cases the patients themselves appeared to appreciate the beneficial effects of the iron injection, and to acquire a feeling of confidence in its power to control the bleeding in a few minutes after its application; a sense of security which, while encouraging to the accoucheur, is not without its beneficial emotional effect upon the patient. I have used the iron in a few other instances, not so typical nor so interesting as to deserve record here; but in all the result has been satisfactory, and unattended by any appreciable after consequences of a disagreeable nature.

I therefore assume from my own experience as well as from that of others that this mode of treating puerperal hemorrhage is both safe and reliable, and under certain circumstances not only justifiable but strongly indicated as one of the most effectual means of rescuing a patient from imminent death.

I am disposed to believe that the action of the iron injection depends not so much upon its direct styptic or hemostatic effect, as upon its influence as a reflex excitor of the incident nerves of the walls of the uterus, and also by directly arousing the peristaltic action of that organ upon which the more powerful muscular actions are as it were based.

One class of cases to which the use of this powerful astringent appears specially adapted are those not uncommon and most troublesome forms of flooding which might be called recurring hemorrhage, where the uterus alternately contracts and relaxes, and where it is difficult to determine when the patient can be pronounced free from risk of further bleeding. Instead of being obliged to grasp the uterus for an hour or two, and feeling afraid to leave the patient perhaps for many hours, the use of the iron at once removes all doubt and difficulty, by inducing firm and permanent contraction.

With regard to the objections which have been raised to the use of the iron injection in flooding, I am not yet convinced that these are to be conceded as sufficiently established by observation to give them weight against the proved safety and efficacy of the treatment when judiciously applied. It has been said that the perchloride acts so powerfully on the walls of the uterus as to leave a layer of dead tissue of some thickness, which is liable to give rise to septicemia. I am not aware that this has been proved by actual observation; it appears to me improbable when the antiseptic property of the solution is considered.

The few reported fatal cases in which the injection had been employed, are not to my mind conclusive evidence of its supposed ill effects.

Deaths from septicemia and other puerperal complications following profuse flooding were sufficiently frequent before the treatment in question was adopted to render it at least doubtful whether the mortality has not been due to other causes than the use of the injection. Granting, however, that in this as in some other powerful remedies employed in circumstances of great and immediate danger to life, there may be a certain possible contingent risk of subsequent mischief, it then becomes a question whether we shall allow a patient to bleed to death before our eyes rather than employ means which we feel confident will rescue her from impending death, although they may subject her to possible future risk.

The mode of applying the remedy has been so fully and accurately described by Dr. Barnes as to render it unnecessary to add anything on that point. It may be well, however, to repeat one condition insisted upon by him, which if neglected will probably cause failure. Before injecting the fluid into

the uterus, all coagula or remaining portions of placenta structure must be carefully removed.

A woman was brought into the hospital literally bleeding to death after an abortion at the fourth month. I opened up the cervix with tents and freely swabbed (I never inject the non-pregnant or immature parturient uterus) out the cavity with the undiluted liq. ferri perchloridi, but the bleeding, so far from ceasing, appeared to flow more freely than ever. As a last resource I introduced a finger up to the fundus and with infinite difficulty scraped off a minute particle of placenta structure, after which the hemorrhage ceased and the patient slowly but completely recovered.—*Obstetrical Journal, June* 1873, *p.* 168.

96.—GALVANISM IN POST-PARTUM HEMORRHAGE.

By Robert Storrs, Esq., Doncaster.

On June 24th, 1873, I was sent for to see a patient in labour, suffering from puerperal convulsions. The convulsions came on during the first stage of labour, and could only be checked by keeping her under the influence of chloroform for some time. Failure of uterine action occurred before the os uteri was fully dilated; but, as it was sufficiently dilatable, the forceps was introduced, and delivery accomplished. Still the uterus did not contract, and, after the placenta was removed, hemorrhage could only be restrained by keeping the hand within the uterus. Grasping and kneading the uterus, cold affusion externally, and injections of cold water per vaginam produced no effect. A dilute solution of perchloride of iron was freely injected into the uterus, but proved ineffectual. My assistant, Dr. Macintosh, then procured for me at my request one of Stöhrer's portable coil machines. An interrupted current of considerable intensity was directed through the uterus, one pole of the battery being applied over the abdominal walls directly over the fundus by means of a curved plate of copper, and the other placed in the cervix. Almost immediately firm contraction took place, and when the current was discontinued the uterus remained securely contracted. The patient made a good recovery.

To those who have witnessed as I have done the confusion and other disagreeable accessories of a case of obstinate hemorrhage after labour, any means that tend to diminish these inconveniences must be most acceptable. The use of galvanism as a means of treatment in these cases, besides its remarkable efficiency as shown as by this case, possesses many other advantages over the means usually employed; for instance, its cleanliness, the simplicity of its mode of application, and the avoidance of those evil consequences in the puerperal state

which are so apt to result from irritation of the uterus by injections, such as that of the perchloride of iron.—*Lancet, Sept.* 6, 1873, *p.* 363.

97.—A CASE OF EXTRA-UTERINE FŒTATION SIMULATING OVARIAN DROPSY.

By JONATHAN HUTCHINSON, Esq., Senior Surgeon to the London Hospital.

[The appearance and physical characters of this case exactly resembled those of ovarian disease except in this particular, that it did not pass into either loin, but occupied the middle of the abdomen. The woman's menstruation had been regular for the last six months, and prior to that there had been a period of nine months' suspension, during which she had supposed herself pregnant.]

On Monday last, the patient having been transferred to my care for that purpose, I tapped the tumour. The patient was at the time on her left side in the recumbent position. The trocar was introduced about two inches below the umbilicus. A dark brown fluid, like linseed tea, escaped, and at the sight of it I at once abandoned all hesitation as to the diagnosis of an ovarian cyst. I have seen just such fluid numberless times from ovarian tumours, and it has rarely happened to me to see anything just like it and in large quantity from other sources. It was the feeling of certainty produced by the appearance and quantity of the fluid which prevented me from making any further examination with a view to diagnosis. After the fluid had been flowing some little time, we observed that the end of the trocar was drawn down towards the pubes, and it became necessary to introduce a probe in order to obtain a full stream. The former fact was explained, as we supposed, by the subsidence of the tumour into the pelvis, and the latter by its multilocular character. The stream became irregular and greatly diminished before we had materially reduced the bulk of the tumour: and, believing that we had established the fact of its being a multilocular ovarian cyst, and therefore a suitable case for ovariotomy, I ceased to be solicitous about getting any larger quantity of fluid away, and withdrew the canula. The total quantity removed was, I believe, between four and five pints. I did not manipulate the tumour much afterwards, but just sufficient to ascertain that it still fluctuated freely in some parts, and felt firmer in others. My intention was to examine it more carefully in a few days, when the trocar puncture in the cyst would be closed. This opportunity was, however, not afforded us. The patient began to suffer from symptoms of

peritonitis almost immediately after the operation, and on the following day she had a high temperature and great abdominal tenderness. The tapping was on Thursday at four o'clock, and she died at half-past four in the afternoon of the following Sunday, exactly seventy-two hours after it. Her symptoms had been distressing sickness, abdominal pain, and rapid pulse. On the Saturday and Sunday her pain had ceased, and on Saturday she had suffered me to handle her abdomen, and assured me that, excepting close to the wound, she had no tenderness. This absence of tenderness is not uncommon in the later stages of peritonitis.

At the post-mortem examination we found the tumour adherent everywhere to the abdominal wall and to the omentum. It had also firm adhesions to the intestine in several places. Everywhere in the abdomen there were evidences of recent acute peritonitis, and in the depending positions there was a considerable quantity of a thin, brown fluid, much like what we had removed at the exploration, only greatly diluted. The tumour itself contained also a large quantity of fluid. At its base, to the right side, was a spot which was almost gangrenous, and which showed a perforation, but I am not quite certain that the perforation was present before the adhesions were torn through. On the opposite side there was another patch in a similar condition. I think there can be little or no doubt that fluid from the cyst had escaped into the peritoneal cavity, but whether through one of these perforations or through the vaginal puncture, or both, it was impossible to say. At the time of the woman's death the trocar wound in the cyst was completely sealed up by adhesions, but it is of course possible, and I think probable, that immediately after the operation it allowed fluid to escape. The walls of the cyst were tolerably thick, but were much softened by inflammation. On the outside, almost everywhere, it was roughened by shreddy patches of cellular adhesion which we had torn through. My first suspicion that it was not an ovarian tumour recommenced when we found that it was in the middle line, that it had no pedicle, and that it concealed the uterus. On cutting into it, a large quantity of fluid of the same character as that removed by paracentesis, only much thinner, escaped, and the body of a macerated fœtus was found at its bottom. The umbilical cord of the fœtus was still continuous with the placenta, but it was rotten, and tore through during removal. The placenta and fœtal membranes, in a much degenerate condition, adhered firmly to the walls of the cyst, which they served to strengthen. The fœtus, considering that, according to the history, it had been dead for six months, showed less signs of decomposition than might have been expected. It

was a female, and at full term. It was not shrivelled, but soddened and flabby. Its eyeballs were quite collapsed, and we noticed, as one peculiarity, that its joints were exceedingly loose, as if it had no ligaments. Its anus was wide open, and would have admitted the finger, and contained a small quantity of meconium. I mention these peculiarities because there was really nothing about the fœtus in general appearance which would have made one think it had been dead more than a fortnight. On further dissection of the parts, the case proved to be one of gestation in the left Fallopian tube. The uterus was pushed over to the right side, and was in close connexion with the walls of the cyst. The left Fallopian tube could be traced for a short distance on the front of the cyst, where it became thinned out and lost. The left broad ligament passed downwards from the front of the cyst, and between its extremity and that of the Fallopian tube was a thickened mass, which might perhaps be the remains of the ovary, but it was not practicable accurately to identify it.

Thus, you will see, we have had to deal with a case of tubal gestation, with the not unusual history of death of the fœtus at the full term, and its retention afterwards. The case differed from the usual course in the distension of the cyst with fluid, which had steadily increased in quantity, and thus simulated ovarian dropsy. I believe that this is very exceptional. I have had to deal with several cases of extra-uterine fœtation, but never with any in which, with a full-sized fœtus, there was any difficulty in discovering the latter. Usually the quantity of fluid is but small, and after the death of the fœtus the cyst contracts on its contents. In this instance, however, the woman was steadily increasing in size, and hence the error in diagnosis. As regards the cause of the patient's death, there can be no doubt that it was due directly to the exploratory tapping, which in all probability allowed of the escape of fluid into the peritoneal cavity. The existence, however, of perforating ulceration of the back of the cyst in two places suggests the probability that, even if the exploration had not been done, the cyst would not have long retained its contents.

Although, probably, as I have just said, it is rare for the cysts of extra-uterine fœtation to contain enough fluid to cause resemblance to ovarian dropsy, yet now and then it does so happen. I have found a case very similar to this in respect to the difficulty of diagnosis from ovarian dropsy, and also in the fact that an exploratory tapping was performed, in a report on the treatment of extra-uterine fœtation which I published in one of the medical journals in August 1860. Curiously enough, it was under the care of a namesake—my

friend Mr. Francis Hutchinson, of Blackfriars. The woman had believed herself pregnant, and, excepting that she had suffered from violent attacks of pain, her pregnancy had passed to its full term without any unusual occurrence. At the end of the nine months labour pains set in, and they lasted more or less for about three weeks. At length the pains ceased, and after this the woman never again felt what she supposed to be the movements of the child. Six weeks later Dr. Ramsbotham, who was, without doubt, the highest authority on this subject, was called in consultation, the woman being ill, emaciated, with an irritable stomach, rapid pulse, and dry tongue. As the tumour was large, and fluctuated freely, the diagnosis of ovarian dropsy was made, and paracentesis performed. About six pints of thick fluid were removed. The trocar puncture never healed, but became a fistula. A fortnight later a lock of fœtal hair presented itself, and thus cleared up the diagnosis. Two months after this, the patient having become still worse in general health, and there being a continuous drain from the cyst, a free incision was made, and the fœtus extracted. The placenta was left to come away afterwards. The patient made a good recovery, and regained perfect health.

Thus you will see that Dr. Ramsbotham was more lucky than myself in that his patient did not die of the exploratory tapping. The difference in our cases probably was this, that in his inflammation had occurred and the cyst become adherent, whereas in mine, there being no adhesions, fluid escaped into the peritoneal cavity. It would be very unsafe to infer from what followed in his case that paracentesis is the best mode of treatment, or even of exploration, in these cases. In some, as has been proved in my case, it is in itself attended by great danger; nor shall I deal honestly with you or with myself if I do not here candidly admit that with due care and patience I do not think that paracentesis ought to be necessary in a case of extra-uterine fœtation simulating ovarian dropsy. If another case of like obscurity should come under my care in which, on examination with the patient on her back, no solid substance could be detected, I would at once make the woman support herself on her hands and knees, and in that position I have no doubt that the fœtus, if present, would be felt. I need not tell you that I exceedingly regret that I did not try this simple manœuvre in the case before us.

We will now turn to consider what our treatment ought to have been had my diagnosis been correct and had I felt certain as to the nature of the case.

In the year 1860 I published the paper from which I have already quoted on this subject, and collected all the evidence I could find, in order to establish principles of treatment. To

this paper I must ask you to allow me to refer. It will be well that we should restrict our attention to cases really similar to the one before us—that is, to those in which the fœtus dies at the full period of gestation, and in which the woman continues to carry it, and, for a time at least, without suffering much in health. Most of you are probably aware that a large majority of cases of extra-uterine fœtation never go on to the full period, but end by rupture of the cyst and hemorrhage into the peritoneal cavity at an early period. There are a few in which this does not take place, and to these I now ask your attention. Several different events were possible in our patient's case. First, the tumour might have become quiet, the fœtus might have shrivelled, and the woman might have lived on to the term of her natural life. Such things have happened more than once, but they are exceptional to the rule. Cases have indeed occurred in which a woman still bearing a dead encapsuled extra-uterine fœtus again became pregnant and bore a living child, or even became pregnant several times in succession. This, however, is less likely to happen if the dead fœtus be of full size than when its death happens early. A second possible event is that the fœtus might have caused inflammation and an abscess, and that thus either the mother's death might be brought about or an opportunity for surgical interference under advantageous circumstances might occur. Thirdly, an operation for the removal of the fœtus might have been performed without waiting. In the paper to which I have referred I have tabulated (chiefly from the excellent monograph of Dr. Campbell) twenty-one cases in which the fœtus remained quiescent; thirteen in which the mother died without any attempt, either natural or by art, having been made for the removal of the fœtus; sixteen in which ulceration occurred and the remains were spontaneously extruded (three deaths); thirteen in which, after an abdominal fistula had formed, the removal of the fœtus was assisted by the surgeon (two deaths); and, lastly, sixteen in which, without waiting for suppuration to occur, the surgeon boldly cut into the abdomen and extracted the retained fœtus. To these, however, I must yet add another group in which the fœtus was got rid of not through the abdominal wall, but by ulceration into the vagina or rectum. The total number mentioned in the table is 102, and of these no fewer than 72 of the patients survived. Probably this ratio of good results is far beyond the real truth, since the cases which recover are precisely those the most likely to excite the interest and perhaps the astonishment of the surgeon, and thus secure themselves record in print. Still, making allowance for this, there is abundant proof that the retention of a fœtus in an extra-

uterine cavity, although very dangerous, is by no means an absolutely fatal condition. As the result of a careful investigation of the evidence, I formed a strong opinion in favour of leaving such cases to nature up to a certain stage. I would not on any account in such a case undertake what is called a primary abdominal section—that is, I would not operate unless an abscess had already formed. The statistics seem to show that whilst, out of the total of 102 patients, only 29 died, out of 16 in which primary abdominal section was performed, more than half died (9). This will give us a mortality of more than one in two when interference occurs before suppuration has taken place, and of less than one in four in those in which nature was allowed time for her own processes.

Let me state clearly that it is not intended to discourage surgical inteference at all stages, for I believe that it is very valuable, and comparatively safe, after inflammation has occurred and adhesions formed. The danger comes when incisions are made into a peritoneal cavity as yet unobliterated with the risk of escape of fluids into it. If, then, in the case which I have been detailing, we had been able to form a confident opinion that it was one of extra-uterine fœtation, I should have advised the patient to wait, and should certainly have declined for the present to interfere. Bearing in mind that the tumour was getting larger from increase of fluid contents, and that its walls had in some parts thinned and softened, I think it not at all improbable that it would have given way into the peritoneal cavity, and thus have produced death; but, on the other hand, we must admit that it is possible it might have suppurated, given way externally, and thus allowed opportunity for the removal of the fœtus, or that the tendency to distension might have come to an end, and that the patient might have lived for some time without serious inconvenience. I do not think that the latter is probable, because the patient was already suffering a good deal of pain and inconvenience from the enlargement of the tumour.

At the risk of somewhat repeating what I have said, I will read to you the concluding sentences of the report to which I have alluded, since they convey in somewhat more detail a statement of my opinions on this not unimportant point in practice. (For extracts, not given here, see Medical Times and Gazette, Aug. 1860.) Those opinions I may, however, briefly sum up in the following practical rule—*that extra-uterine fœtation cysts ought not to be meddled with in any way, either by puncture or incision, until suppuration has occurred and an abscess fistula has been formed.—Lancet, July* 19, 1873, *p.* 71.

98.—GASTROTOMY IN A CASE OF EXTRA-UTERINE GESTATION.

By F. H. Maberly, Esq., Surgeon Chloroformist to the Dental Hospital, Birmingham.

Mr. Bailey, surgeon of this town, requested me, on June 21st, to see Mrs. —, a young woman aged 27, mother of two children, the youngest being two years old. I found her suffering from excruciating pains in the abdomen, and constant sickness. Her face was pinched, and expressive of great anxiety; the pulse rapid and feeble. On examining the abdomen, I found its walls exquisitely sensitive, so that no pressure could be borne; yet, on the right side, could be seen and felt a solid substance, springing evidently from within the pelvis, rising to a level with the umbilicus, and slightly crossing the median line to the left. The history showed that this substance had been forming for about six months, during which time she had constantly suffered with severe intermitting pains of a bearing-down nature, together with frequent nausea and vomiting; the bowels had been very costive, with tenesmus coming on at intervals, and sometimes she had had complete retention of urine for some hours; all these symptoms had become very much aggravated about ten days before I saw her. There were no breast-signs of pregnancy, and she had never felt any movements. A vaginal examination with the finger revealed the uterus carried high up, and greatly anteverted. The menstrual discharge had been very irregular from the commencement of the symptoms. She was ordered full doses of opium, and a turpentine stupe to be applied over the abdomen. Next day she appeared somewhat better, and I did not see her again till the evening of June 26th, when I found the symptoms, if anything, increased in severity, and she had had some rigors. As no efficient examination could be made without the use of chloroform, I determined to place her under its influence on the following day; and having done so I was enabled, by the additional evidence thrown on the case, to express a decided opinion that it was one of extrauterine pregnancy; for not only did the history and symptoms entirely favour this view, but nearly every other possible disease could be excluded.

Owing to the extremely precarious state of the patient, it was thought advisable, before proceeding to operate, that another opinion should be had, although the patient and her friends were most anxious that what was necessary should be done at once, notwithstanding only a very slight chance of recovery was held out to them. I was extremely fortunate in obtaining the opinion of my esteemed friend, Dr. Savage, the same afternoon, when he agreed that immediate operation was

the proper course. I therefore, in the presence of Mr. Bailey, Dr. Savage, Mr. Priestley Smith, who kindly gave the chloroform, and two others, performed gastrotomy.

An incision about four inches in length was made in the median line between the umbilicus and the pubes. Dissecting down through the linea alba, the peritoneum was reached, and opened upon a director, when a quantity of dark blood began to flow from the abdominal cavity. The tumour was now exposed; and, as it was somewhat elastic to the touch, an ovarian trocar, for safety's sake, was thrust into it; but, as no fluid escaped, I withdrew the instrument, and introduced my finger through the opening it had made, when, to my extreme satisfaction, I found my diagnosis to be correct, as I at once felt the limbs of the fœtus. Opening the cyst, I removed it by the feet; and, in doing so, found the head lodged deeply in the pelvis. The fœtus weighed about a pound and a half. All blood was removed as completely as possible from the abdominal cavity. Its effusion had been caused by a rupture in the cyst, which no doubt occurred at the time when the symptoms increased in severity—viz., about ten days before I saw her. The cyst-wall was then stitched to the abdominal parietes, the placenta being left *in situ*. A pad of wet lint placed over the wound, covered with tenax, and a bandage, completed the operation. The patient was then removed to bed; and a small quantity of brandy was administered, as she appeared faint. She soon, however, became very restless, and in about three hours breathed her last. The next day, I was permitted to examine the parts. I therefore enlarged the abdominal wound, and exposed the cyst, &c. The peritoneum in the right lumbar region was covered with blood, which had evidently been effused for some days. Adhesive inflammation had been set up about the part of the cyst where the rupture had occurred, gluing it to the ascending colon, and slightly to the parietal peritoneum. The omentum was likewise covered in patches with discoloured blood, and was of a greenish cast in many places. The cyst was found to be connected with the right Fallopian tube, and no trace of an ovary could be found on that side. The uterus was rather enlarged. The ovary, round ligament, and Fallopian tube on the left side, were all healthy. —*British Medical Journal, July* 26, 1873, *p.* 81.

99.—ON THE OPERATION FOR RUPTURE OF THE PERINEUM AND PROLAPSE OF THE WOMB.

By JAMES FOWLER, Esq., Surgeon to the Wakefield Hospital and Dispensary.

To say nothing of the comparative difficulty of even ordinary labour; when the vaginal orifice is small and unyielding,

the head of the child large or extensively ossified, or the presentation and birth otherwise abnormal, even with the greatest skill and attention, laceration of the perineum is a common accident—in some cases it is inevitable; and lacerations of the whole length of the perineum, even though not involving the rectum, if not, as frequently, a source of distress and misery at once, are at least liable at any time to become so. Besides the inconvenience and discomfort incident to the patient herself, and the prolapsus, rectocele, or cystocele, which are almost sure sooner or later to follow, and which furnish so large a proportion of our hospital cases, the attachments of the constrictor vaginæ and other muscles of the perineum being torn through, the normal conditions for proper intercourse no longer exist. Accordingly, it is now generally admitted that ruptures of the perineum should be dealt with at once, on the completion of labour, or at least as soon after as practicable. For several years this has been my own practice, and with the best results. I have brought the edges of the wound together by means of deep quill and superficial sutures; kept the bowels confined as long as necessary; drawn off the urine regularly with a catheter; kept the patient on a water pillow; had the vagina daily washed out with water and disinfectants; and have been rewarded by always having had union follow—a union the strength of which has in several cases been tried by subsequent labour, and remained firm and elastic. A perineum will, I know, sometimes give way in subsequent labour, however carefully repaired. A lady, not at that time a patient of mine, whose perineum was ruptured in her first confinement, was operated upon in London ten weeks afterwards, in consequence of the great suffering which ensued, by one of the most distinguished surgeon-accoucheurs then living; but four years afterwards, notwithstanding every precaution, it gave way during her next labour. I brought the wound together in the usual manner at once, and secured capital union, avoiding in that manner all the inconvenience and trouble she had previously experienced; but there has been no labour since to test the strength of the parts.

A perineum, again, though apparently well repaired and remaining good through after labours, will not always prevent subsequent prolapse of the womb; relaxation of the vagina and ligaments of the womb, perhaps, ensuing long after the operation has been performed. In some cases, possibly, the removal of mucous membrane may not have been as free as it well might have been; in others the union may not have been carried sufficiently high. But from whatever cause the failure of the operation may have arisen, the fact that there has been a failure at all has repeatedly been urged as a practical objection against its performance. It is in order to show,

on the other hand, what complete relief may be afforded by a judicious operation, that I give notes of the following case.

J. R., aged fifty-six, was confined for the fourth and last time eighteen years ago. She had a slow labour. The child was very large, and instruments were sent for; but before they arrived, all was over naturally. She felt afterwards unusual pain and tenderness of the perineum, and had much scalding in making water. At the end of a fortnight, however, she got up, and did not at once feel much inconvenience. Fourteen years ago she ceased to menstruate. Eight years afterwards she began to have great pain in the back on standing, with occasional bearing-down; and four years later, on lifting a recently filled water-filter, "felt something give way in her inside," and the womb appeared externally. Since then she has been unable to keep it up either with pessaries or any other contrivance. She has had to give up washing; has been unable even to iron a shirt; has not been able to stand during the singing of a hymn in a place of worship; has been unable to walk more than a quarter of a mile at once at the furthest; for weeks together has often been unable to get out of the house at all; has suffered almost constantly from leucorrhœa; has often been faint, and sick, and off her food; and, more than all, has had to suffer the domestic misery and distress arising from such a state of things.

Such was her history when she came into our hospital on the 3rd of April, 1871. Her countenance was blanched and expressive of great anxiety. On making an examination, the womb was found extensively prolapsed; there was an ulcer, about as large as a shilling, on the os; there was a great deal of leucorrhœa; and the perineum was ruptured backwards as far as the verge of the anus.

Having been placed under the influence of chloroform, in the position for lithotomy, the sides of the fissure were completely denuded from the point where the posterior commissure of the vulva should naturally exist, backwards and upwards towards the rectum, which was next freely denuded in like manner. Immediately in front of the rectum the denudation was carried somewhat higher than usual, in order to produce a certain amount of subsequent contraction of the vagina, and thus prevent the redescent of the womb. Having waited patiently now for some time, and applied ice until the bleeding had entirely ceased, two strong waxed-silk sutures were inserted by means of a long curved needle, and firmly tied on quills on either side the wound, for deep union; and three of stout silver wire, for superficial union of the edges of the skin along the middle line of the perineum. On passing the

forefinger into the vagina, apposition of the two sides of the fissure was found to be complete.

Placed on her left side on a water cushion, with the knees tied together and flexed on the abdomen, the treatment was the same as after operations generally, except that, in addition, the bowels were confined with opium, the urine regularly drawn off every six hours with a catheter, and the vagina washed out every day with a stream of warm water and Condy's fluid. I have no regular time for removing the sutures, but in this particular case the deep ones were taken out ninety hours, and the superficial ones a hundred hours after the operation. Union was found to be complete and firm. On the seventh day the bowels were relieved by castor oil and an enema, so as to cause as little disturbance of the new perineum as possible. In order to avoid any possible strain, and to allow the swollen and ulcerated womb to recover its normal condition, and its relaxed ligaments to strengthen, bed was enjoined for three weeks longer; and at the end of the month she drove home in a cab.

For a long time she could not believe that she was really cured, but gradually felt her way about her household duties. The womb has never been down since, nor has she ever had the slightest feeling of pain or inconvenience. She can wash a fortnight's wash and iron and clean for a family of four regularly now without assistance, fetching and carrying water, emptying the tubs, &c., all by herself. She also walks to Wakefield every week in the middle of the day to market—a distance of four miles, does her shopping, and then walks back again in the evening, without the slightest feeling of local weakness; and gets along faster than one of her years and figure (rather stout) might be expected to do. She can also stand quite comfortably for any length of time. Her general health is as good as possible, and she has never had any leucorrhœa since she left the hospital, now more than two years ago.

By an operation of this kind it must be admitted therefore that the evils of a torn perineum may be completely remedied. If not raised to an ideally perfect condition, the perineum thus treated is at least brought back to the state in which it was before the accident, which is sufficient—at least in the great majority of cases—for all practical purposes.—*Lancet, July* 19, 1873, *p.* 79.

100.—ON INJURY TO THE RECTUM IN PARTURITION.

By CHRISTOPHER HEATH, Esq., Surgeon to University College Hospital.

Every now and then in primiparæ, and especially in women advanced in years, when the head of the child is on the peri-

neum, the perineum gives way. The tear may be simply in the perineum, or it may extend through the sphincter ani and lay open the rectum. This is of course very serious both for the patient and also sometimes for the doctor; but it is to be remedied, and usually remedied with success if the case is treated at once. You have a tear through the anterior part of the sphincter, and you have to bring the parts into apposition so that they may heal while fresh. The best plan is the ordinary method pursued for ruptured perineum with deep sutures. It is not necessary to have recourse to the old-fashioned quill suture, which is apt to give rise to ulceration by the pressure exerted upon the edges of the wound. Take an ordinary good, large, curved needle, and pass it in deeply on each side of the ruptured perineum, going well down to the bowel, or even into it. Use a good piece of thick wire, twist the ends together over the front, and get the parts drawn well together. You can leave these sutures in much longer than you can leave quilled sutures in, and with less damage to the skin and surrounding parts.

But if it is a case of rupture *into* the rectum you must do something more. It is no doubt owing to contraction of the sphincter ani that some of these cases do so badly. In former times, before the operation on the perineum, Mr. Copeland Hutchinson, a rather celebrated surgeon in his day, proposed to treat the cases simply by dividing the sphincter ani on each side. He was perfectly right in his view, and I believe he carried it out in one or two cases; and so also did Mr. Mayo. The patient lay afterwards with the legs bound together, and all fæces passed involuntarily out of the sphincter without the sphincter contracting; and they found that they did occasionally gets the parts to unite. We have nowadays the method they practised, but, in addition, we put in deep sutures; and therefore, having done that, I advise you to pass the finger into the anus, and, with a blunt-pointed straight bistoury, to divide the sphincter on each side. It is not sufficient to divide it on one side, because the sphincter is really a double muscle. Keep the patient quiet and lock the bowels up with opium, and you will probably have a good result; at all events it is the best chance for the patient.

It unfortunately happens, however, that you do not always obtain a complete result. The patient recovers with an apparently sound perineum, but discovers that she passes fæces and flatus per vaginam. It is a very painful thing for a woman to find that flatus escapes without her will, for she is thereby shut out from society. Of course there must be a small fistula if simple flatus passes; if it be larger you have in addition fæcal matter passing through into the vagina, and thus

escaping. We have of course here the condition known as recto-vaginal fistula, and it is easy enough to see, if large, by putting the patient into the lithotomy position. If minute, however, giving passage only to flatus, it is not always easy to find it. You may have to hunt for some time for it, as in the case of fistula in ano. Having found the fistula, the object is to close it. If minute, you can often effect this by using the wire made hot by a galvanic battery, but if large you must pare the edges and sew up the opening. My own personal experience is unsatisfactory, and surgeons who have had a more extended experience of recto-vaginal fistula find it a difficult thing to cure. The reason is this, that the tissues upon which you have to operate to get the raw surface to unite are so exceedingly thin that to pare them so as to get a broad raw surface to unite is very difficult to manage. I have notes of two cases that were operated on which were relieved, but I am sorry to say they were not cured. You will generally find that these operations need to be repeated three or four times or more before you can completely close the opening. Briefly, one case was this. The patient was a woman, aged twenty-three, having two children. Her first confinement was difficult; she was delivered by instruments, and ever since she has been able to get about she has found her motions passing by the vagina. I may remark here concerning the use of instruments, that both the surgeon and the instruments are often blamed unnecessarily. It is the common story, "Mr. So-and-so used instruments, and the woman has never been well since." It is not the fault of the surgeon or of the instruments, but it is commonly the fault of an ignorant midwife who has allowed the head to rest so long upon the parts as to make them slough, as is seen also in vesico-vaginal fistula, a result which is more frequent than recto-vaginal. The woman came into the hospital, was operated on here, and was improved, but was not cured.

The other case was that of a woman aged thirty-one. In her first confinement her attendant put on the forceps, and in delivering the child the perineum was torn. He immediately put the parts together, and put deep sutures in, and treated the case *secundum artem*. But she had an opening into the rectum just above the sphincter, through which both fæces and flatus passed. I performed an operation on it with great care, making the edges of the fistula raw, and not merely paring the edges themselves, but taking a good broad piece off, and then bringing the surfaces together with sutures. It is very difficult to do it effectually, for there is a great tendency for it to break down again the first time the patient has a motion. This unfortunately happened in this patient. We locked up the bowels for

some time, but they acted rather too soon, and we had all our work undone, and she is coming in again to have another operation performed.—*Lancet, May* 24, 1873, *p.* 726.

101.—ON AN IMPROVED METHOD OF EXAMINING AND SURGICALLY TREATING THE FEMALE RECTUM.

By Dr. HORATIO R. STORER, Surgeon to St. Elizabeth's Hospital for Women, Boston, U.S.

[This paper was elicited by a lecture of Mr. C. Heath's, in the Lancet for January 18th. Mr. Heath, after speaking of the advantage of administering an enema previous to examination, and having the patient force out the lower gut by subsequent straining, mentions the ordinary digital method, employment of the common anal specula, and that of the endoscope.]

He says also that it is advisable in examinations of women to pass the finger into the vagina as well as the rectum, since "a rectal stricture, for instance, can often be much more thoroughly examined through the vagina than is possible if the examination is confined to the rectum itself." Here, however, he stops, just short, it will be perceived, of accomplishing the manœuvre now to be described.

For many years I have been in the habit of attaching importance to the condition of the rectum in uterine cases. My attention was first drawn to their correlation by Sir James Simpson during my residence in Edinburgh in 1854-56, and it was subsequently increased by Mr. Baker Brown's admirable chapters upon the effect of morbid conditions of the rectum upon uterine disease and of those of the uterus upon rectal affections. Finding the ordinary methods of exploration, such as Mr. Heath has described, tiresome, imperfect, and in every way unsatisfactory, it occurred to me that *eversion of the anus and lower rectum by digital pressure from within the vagina* might give better results, and such has proved to be the case.

As I have described the process at some length in a series of papers upon the relations of the rectum to uterine disease, published several years since in the American Journal of Obstetrics and Diseases of Women and Children (New York), and have also spoken of it in the Journal of the Gynæcological Society of Boston, it will be unnecessary here to do more than state some of the results that it enables us to obtain. As to the manner of effecting the eversion, it is only requisite, the patient being upon her side, to introduce the finger into the vagina a sufficient distance, and then to press steadily backwards and downwards upon the sphincter ani; this muscle will relax in consequence, and allow the rectal wall for a large por-

tion, and in some cases the whole of its circumference, to turn itself outwards through the anal orifice, very much like the everted finger of a glove. Should the sphincter ani be hypertrophied or irritable and resistant, as in some cases of fistula, &c., and digital pressure from within the vagina prove insufficient to produce eversion to the extent that is desired, it may at once be accomplished by previously rupturing the sphincter by rapid dilatation with the thumbs, as suggested by Récamier for the treatment of anal fissure. This is a very simple procedure, unattended by any subsequent ill-effects, and I have now resorted to it for many other indications and in a very great number of cases.

1. *Artificial anal eversion for purposes of diagnosis.*—Every one familiar with rectal surgery knows how important it is to get at the precise character and extent of the existing disease, whatever that may be, and that it cannot be accurately effected by the touch alone. Were the rectal mucous membrane unbroken by folds this remark would still be true, but rugous as it is, the most skilled touch will often fail to detect quite extensive lesions. The laryngoscopist who should undertake the discovery, much more the differential diagnosis, of his special class of affections by the finger would be justly laughed at, and attempts to treat them through specula as awkward and unsatisfactory as are the best forms employed for the rectum would be considered even more blameworthy. It will probably not be long before the usual methods of rectal manipulation are considered just as unsatisfactory by surgeons.

By the method now described, one is able to ascertain the exact position of the internal orifice of fistulæ — upon the necessity of discovering which Mr. Heath lays much stress in a subsequent lecture (Lancet, Feb. 22nd, 1873), although he can only suggest "hooking" for it by a bent probe,—the site and extent of ulcers, whether specific or otherwise, the attachment and character of internal hemorrhoids, the presence of minute foreign bodies that may have become imbedded in the mucous membrane during their passage from above, &c., to an almost indefinite extent.

2. *Artificial anal eversion for purposes of treatment.* — The extent of the new field here opened to the surgeon is even greater than that already stated. In addition to the fact that he is enabled to perform the usual operations with greater exactitude, such as cauterisation by acid nitrate of mercury or the hot iron, without injuring contiguous healthy membrane, as is else almost necessarily done; the excision of internal hemorrhoids, which is so preferable in every respect to ligation, now that even the slightest risk of subsequent concealed hemorrhage can be so surely prevented, &c., it becomes easy to

accomplish others hitherto almost impossible, at least so far as the obtaining a perfectly successful result. Take, for instance, imperforate anus in the female. In these cases, the pouch of meconium at the extremity of the gut almost always projects against and beneath Douglas's fossa. It will be found that the infant vagina admits a finger without difficulty, and that by a little discreet pressure thereby from within the pouch can be so flattened and carried downwards that anal incision into it can be made infinitely more accurately than has yet been possible; and also that the most important step of all in the operation, the stitching of the extremity of the mucous membrane to the external integument, can be effected without difficulty. In cases where a congenital vaginal *cloaca* exists, the operation for its effacement and the completion of the natural canal can almost as easily be performed.

It is hardly necessary to refer to the cases, not so very rare, especially in the insane, where foreign bodies of large size have been introduced into the rectum, or to the very common ones of its impaction by hardened fæces. In both these classes outward pressure from within the vagina accomplishes removal in a fraction of the time otherwise required.

It may be well to add, in the present paper, that visual exploration of the upper rectum, even for some distance within the sigmoid flexure, is perfectly practicable without the endoscope, and to a much greater extent than can be accomplished by that instrument, where the sphincter ani has been relaxed by forcible dilatation in the manner above referred to. A full-sized vaginal bivalve speculum—not the little anal affair referred to by Mr. Heath—can then be introduced without difficulty, and the upper portion of the canal is rendered as accessible for all purposes of diagnosis and operative treatment as the vaginal cul-de-sac itself.

This last hint will not be lost upon those engaged in the treatment of the rectum, and even prostate and bladder, in the male, for I have found that by forced anal dilatation, and the use of a large-sized vaginal bivalve—that of Cusco, for instance—it has become possible also to revolutionise practice in this direction, as well as in the treatment of women.

With regard to the priority of the suggestion of anal eversion by pressure from within the vagina, I may state that not only has it been new to the many physicians who have seen me perform it in hospital and private practice during the last ten years, but that no trace of it could be found among leading British and Continental gynæcologists by one of my pupils, Dr. Stone, now of St. Paul, Minnesota, whom I had requested to make special inquiries as to this point during a late trip to Europe.—*Lancet, May* 31, 1873, *p.* 766.

102.—OPERATION FOR RECTO-VAGINAL FISTULA.

(Under the care of Dr. DEMARQUAY, Paris.)

In the course of a recent visit to the wards of Dr. Demarquay, at the Hospice Municipal, we witnessed the above operation, some of the circumstances of which are of interest. The operation was very ably performed on a woman aged thirty-four years, in whom an incision had been accidentally made by a surgeon in operating for polypus through the recto-vaginal wall in about the situation of the mesial line.

Operation.—The patient was laid on the left side and put under the influence of chloroform. The duck-bill speculum was introduced into the rectum, so as to explore the region. This quite an original feature in Dr. Demarquay's operations on the rectum; he always uses a small duck-bill speculum as being infinitely superior to the common speculum ani in affording light and space. The incision was then disclosed to the view, measuring about two centimetres in length, and easily admitting the operator's fingers. Dr. Demarquay then slit the sphincter ani and rectum towards the coccyx with a bistoury. This was done with a twofold object—to give room for performing the operation, and to facilitate later the passage of the fæces, so that they might not bear too directly on the situation of the repaired fistula. Slight venous hemorrhage being stopped, the patient was then placed on her back, with the pelvis overhanging the edge of the pad, and the legs well held up by two assistants. A duck-bill speculum was introduced, and placed against each side of the vagina, so that the lateral walls were widely stretched open. The lips of the fistula were then largely pared, Dr. Demarquay insisting on the importance of touching up the edges of the fistula over an extended surface. Arterial hemorrhage was stopped by means of Kœberlé's pincers (with notches in the handle, so that the pincers hold on by themselves) and two ligatures. Seven sutures, with silver wires, were then applied; and, on the vagina being carefully washed out by means of an injection, the lips of the incision were seen to be nicely and exactly adapted. Whilst the sutures were being applied, a large duck-bill speculum, passed under the pubes, held the labia widely open, whilst a small sponge at the end of a pair of pincers was introduced into the rectum in order to hold up the lips of the incision, and to prevent the posterior wall of the rectum from being taken up by the needle.

The treatment consisted of warmth, absolute quiet, opium, and sounding for urine every six hours.—*Lancet, May* 17, 1873, *p.* 698.

103.—ON THE MECHANISM OF THE PRODUCTION OF CERTAIN DISPLACEMENTS OF THE UTERUS.

By Dr. JOHN WILLIAMS, Assistant Obstetric Physician to University College Hospital, London.

In a paper recently published in the Lancet, the limits of the changes occurring normally in the unimpregnated uterus were defined. In no one of those positions were any of the so-called uterine ligaments put on the stretch by reason of the position assumed by the uterus. The utero-sacral and utero-vesical ligaments were, however, put on the stretch when the rectum and the bladder were both distended. The stretched condition of the ligaments was not due to the position of the uterus, but to the distended state of the organs with which they are connected. Of course, when the rectum alone is distended, the utero-sacral ligaments only are stretched. It follows, then, that, whatever may be the use of the so-called uterine ligaments, it is not to limit the antero-posterior movements, ascent and descent, of the unimpregnated uterus.

Having premised this much, we are in a position to investigate the mode of production of abnormal positions of the uterus. In the present paper the mode of production of antero-posterior displacements—anteflexion, anteversion, retroflexion, and retroversion—will alone be considered.

A change in the position of any body can be produced by mechanical force only, and therefore the mode of production of displacements of the uterus is a question of mechanics.

The uterus is a semi-rigid tubular organ, having walls traversed by canals which anastomose freely, and which are filled with fluid. In what ways is it possible to produce a flexion of such an organ? A bend in an organ so constructed may be brought about in any one of the following ways:—(*a*) By a force acting at one end in the direction of the flexion, the other end being fixed; (*b*) by a force acting at each end, the middle being fixed; (*c*) by a force acting at a point situated between the two ends, the latter being fixed; (*d*) by gravitation, one end being fixed, when the organ has been lengthened without proportionate increase in the thickness of its walls, or when the canal has been enlarged, the thickness of the walls remaining the same, or when the thickness of the walls has been diminished, the canal remaining the same, or when the fluid contained in the canals anastomising in the walls of the organ has been removed or greatly lessened, or when the consistence of the material forming the walls of the organ has been so changed as to become soft.

What are the mechanical forces which can act on the uterus? The force of gravitation is one which is quite insufficient to

produce a flexion of the unimpregnated uterus in any condition of the organ which I have met with. As a cause of flexion, indeed, it may be utterly ignored, but as a cause of version it is the most prevalent.

The force generated by the distending rectum acts invariably on the under and posterior surface of the uterus in such manner as to raise and push the organ forwards.

The distending bladder produces a force which always acts on the anterior surface of the organ, and pushes backwards at the same time that it slightly elevates the uterus.

The force of the diaphragm communicated from above through the abdominal viscera may strike the fundus directly, or may strike the anterior or posterior surface of the fundus—depending on the position which the organ occupies at the time. This force is directed downwards and forwards in the direction of the continuation of the anterior surface of the spinal column; so that when the fundus is raised above or in front of the plane continuing the anterior surface of the spinal column the posterior surface of the fundus will be struck by the diaphragmatic force, and anteflexion becomes possible. If, however, the fundus of the uterus be below or behind the before-mentioned plane, the force of the diaphragm will act on its anterior surface, driving it further back into the hollow of the sacrum.

The force produced by the abdominal muscles is directed backwards, and can usually act on the anterior surface of the uterus only, pushing it back into the sacral hollow. When the uterus is elevated, however, the force produced by the action of the abdominal muscles strikes the superior surface of the fundus, driving the uterus lower into the pelvis.

The other forces which may act on the unimpregnated uterus are: the weight of the superincumbent viscera, the force of inertia in falls, jolts, &c.

Adhesion of the fundus to the peritoneum in front of or behind the uterus is not very uncommon. Occasionally the organ is closely bound down in a retroflexed position, but usually the bands forming the adhesions are of considerable length, and they allow of considerable movement of the fundus. I think it is very improbable that adhesions ever cause flexion; in most if not all cases where the uterus is closely bound down the adhesions and the malposition were simultaneous in their origin; the adhesions, however, served to maintain the malposition.

Anteflexion.—When the uterus lies in the pelvis below and behind a line drawn in continuation of the anterior surface of the spinal column, the only forces that can act on its posterior surface are the distending rectum and growing tumours.

Growing tumours may cause anteflexion, but the distending rectum cannot. When, however, the uterus is elevated above or in front of the line drawn in continuation of the anterior surface of the vertebral column, other and most powerful forces may act on the posterior surface of the organ, and push the fundus forward. These forces are, the force of the diaphragm, the weight of the intestines, and inertia from falls, &c. When the rectum is full and the bladder is full the uterus occupies a higher position in the pelvis than in any other condition of these organs. It is raised above or in front of the imaginary continuation of the spinal column. In this position the fundus is free to move, and is quite unsupported anteriorly, for there is a space between the anterior wall of the uterus and the posterior wall of the distended bladder. The cervix, however, is fixed, and cannot be moved in any direction. This is brought about by several agencies: the rectum is distended, and resists any movement of the cervix in a posterior direction; the anterior wall of the vagina and the base of the bladder are stretched to their full tether and prevent any backward movement; the utero-vesical ligaments are stretched, and the connective tissue medium uniting the cervix of the uterus to the posterior angle of the bladder is put to the full stretch, so that the neck of the uterus is firmly bound to the bladder, and all movement is prevented. This is one position of the uterus in which anteflexion can take place; indeed, it is the only one, for in all other positions of the organ in which a force can be exerted on the posterior surface of the fundus of the uterus, so as to drive it forwards, the cervix is freely movable. A strain, or severe exertion, or a fall, when the uterus is in the above position, will produce antiflexion.

A fibrous tumour growing in the posterior wall of the uterus, involving the cervix and a great part of the posterior wall, but leaving a free portion near the fundus, may enlarge, fill the pelvis, elevate the uterus, and fix the lower part of it, so as to bring the organ into a condition in which anteflexion is possible.

Adhesions between the anterior wall of the uterus and the bladder are not very common. In the cases of adhesion which I have seen the connecting bands were of such length that it would have been impossible for them to have caused flexion. In one such case anteflexion existed. The adhesions were over an inch long, and probably came on from an attack of pelvic peritonitis after the flexion had occurred.

Anteversion.—There is a form of anteversion described, but said to be very rare, which is called "complete anteversion." I have never seen such malposition. There are two ways in which it is possible for it to occur. I pointed out in a former paper that when the bladder is empty and the rectum full the

uterus is anteverted—the whole length of the organ lying on the collapsed bladder and the anterior wall of the vagina, the fundus being immediately behind the symphysis pubis. In the so-called complete anteversion the uterus lies on the bladder and anterior wall of the vagina, but the fundus is at some distance behind the symphysis. This may be congenital or acquired. In the former case the anterior wall of the vagina is naturally too long. In the second case the anterior wall of the vagina has been artificially elongated. Dr. Marion Sims relates a case in his "Uterine Surgery" which favours this view. He performed an operation in order to shorten the anterior wall of the vagina, and the malposition was cured. The acquired elongation of the anterior wall may possibly occur after pregnancy, but more probably from great exertion when the uterus is already anteverted—that is, when the bladder is empty and the rectum is full.

Retroflexion and Retroversion.—When the uterus occupies a position below or behind the plane which would form a continuation of the anterior surface of the spinal column, the action of the diaphragm, of the abdominal muscles, weight of viscera, inertia from falls, &c, is to push the fundus backwards and downwards. When the rectum is empty the uterus occupies such a position; the bladder may be full or empty. The condition of the rectum then, is the important matter in the consideration of retro displacements. The rectum may be empty under a variety of circumstances. The bowels may be regular, and then the rectum is occasionally full and occasionally empty. When the rectum is empty the uterus is somewhat retroverted. Any sudden exertion, strain, or fall under these circumstances will increase the retroversion, or may even produce retroflexion. But the bowels may be constipated—that is, they may be open every three or four days,—and there is much straining at stool; and what is passed consists of a few small hard fæcal masses. The condition of the rectum in these cases is one of comparative emptiness; it is not at all distended: indeed it contains only a few small, hard masses of fæces lodged in its folds, and great force is necessary to dislodge them. (This is the usual condition of the rectum in patients suffering from constipation and passing such stools as described; at the same time there are cases of constipation in which the rectum is much distended, but the state of the stools in these cases is totally different from the cases stated above. Of cases of constipation, some of the most obstinate are those in which the rectum is immensely distended.)

The physiological actions performed at a healthy stool (the rectum and bladder are always full) are, first of all, evacuation of the bladder: as the bladder collapses the uterus becomes

anteverted; the rectum is then emptied, and in consequence, the bladder and the uterus sink somewhat lower in the pelvis; and as soon, probably, as the woman begins to walk the uterus falls to it lowest normal position—the cervix comes to rest on the top of the inclined plane of the vagina. When, however, there is constipation with much straining at stool this does not happen. The rectum is from the first comparatively empty, the bladder is full and the uterus is somewhat retroverted. The first physiological act performed at stool is evacuation of the bladder. As the rectum is almost empty the uterus does not become anteverted as the bladder collapses, as it does in health. It was from the first somewhat retroverted, and it only descends slightly by the evacuation of the bladder: it still remains retroverted. Having emptied the bladder, the person proceeds to empty the rectum, which is by no means easy. She strains, and the uterus becomes still more retroverted. She strains again until a few small, hard masses are passed. The uterus by this means is driven still lower, especially the fundus. The uterus lies now with the fundus directed backwards and somewhat downwards; the cervix is directed forwards and slightly upwards; and the uterus, at the junction of its neck and body, rests on the posterior wall of the vagina, so that the organ cannot descend lower than as a whole. Here we have the middle point of the uterus fixed, with both extremities movable; for the fundus may descend until it reaches the bottom of Douglas's pouch, and the cervix may descend, or it may ascend until the anterior vaginal wall is fully stretched. This position of the uterus is one of great retroversion.

During the act of straining (the uterus having been driven into the above position) the mass of the intestine acted upon in front and above by the diaphragm and the abdominal muscles is driven into the pelvis, and acts like a wedge. One of two things may happen. Should the anterior wall of the vagina be sufficiently long to allow the fundus of the uterus to be pushed to the bottom of Douglas's pouch, that may take place. The fundus of the uterus by this means becomes fixed, and the middle point is also fixed; any further straining will bend the cervix downwards into the vagina, and retroflexion is the consequence. But the anterior wall of the vagina is, as a rule, not so long as to allow the cervix to ascend to such a degree as to permit the fundus to descend and become fixed in Douglas's pouch. In this case, as soon as the anterior wall of the vagina has been fully stretched, the cervix becomes a fixed point, the fundus is by further straining driven lower down into Douglas's pouch, and retroflexion is the result. The fundus of the uterus with its broad ligaments having been

driven into Douglas's pouch, the veins of the latter become constricted by the utero-sacral ligaments, which form the upper margin of the pouch, and in this manner is caused the congestion of the body of the uterus so commonly associated with retroflexion of the organ. In cases of simple anteflexion of the most acute kind occurring in virgins, the enlargement of the body of the uterus is not marked. In women who have borne children, however, there is often considerable enlargement; but this enlargement is due to complications accompanying the flexion.

In many cases of retroflexion in which there is difficulty in replacing the body of the retroflexed uterus, the cause of the obstruction is to be looked for, not in adhesions, but in the state of the uterus itself and of the ultero-sacral ligaments. The fundus and part of the broad ligaments in these cases have been for a long time lodged in Douglas's pouch; the utero-sacral ligaments compress the broad ligaments and the lower part of the body of the uterus; the portions of the broad ligaments immediately adjacent to the uterus and the uterine body itself are mechanically congested and swollen, so as really to be too large to pass up between the somewhat stretched sacro-uterine ligaments. If in some of these cases the cervix be pushed back, the utero-sacral ligaments are somewhat relaxed, and the fundus of the uterus can be pushed up between them, and the displacement reduced.

Diminution in the amount of blood in the vessels of the uterus is little, if at all, known, though congestion—a condition most unfavourable to the production of flexion—is said to be not uncommon. Probably those curious cases of flexion which are sometimes directed forwards and sometimes backwards are due to a deficiency of the circulating fluid in the uterine vessels. This view is favoured by the fact that such cases occur almost invariably in very anæmic women.

Softening of the tissue of the uterus is a condition of the organ, the pathology of which has yet to be elucidated. I have seen it only in cases of pregnancy, some cases of fibrous tumours, and some cases where the uterus had contained foreign bodies. This would point to its being due to rapid growth or to rapid change of tissue. I have never seen a uterus whose tissues were so soft that it could be said that the soft condition was capable of causing flexion; nor have I seen a flexed uterus so soft that the flexion could in any way be ascribed to that condition.

Anteflexion of the uterus, then, is possible only when the bladder and rectum are both full enough to raise the fundus of the uterus above and in front of an imaginary plane continuous with the anterior surface of the spinal column.

Anteversion is only possible when the rectum is full and the bladder empty, except when the uterus is considerably elongated, in which case it is the permanent condition. The so-called "complete anteversion" is only possible when the anterior wall of the vagina is of unusual length.

Retroversion is only a step towards retroflexion. Both conditions may be produced when the rectum is empty, and that is a necessary condition for their production.

Elongation of the uterus, enlargement of the uterine canal, and diminution of the thickness of the uterine walls, are questions which can be discussed only in connection with pregnancy and fibroid tumours.

On the influence of the above conditions in the production of displacements, as well as on the pathology of flexions generally, I hope to publish on a future occasion.—*Lancet, Aug.* 30, 1873, *p.* 298.

104.—ON FIBROID TUMOURS OF THE UTERUS.

By Dr. ALFRED MEADOWS, Physician-Accoucheur to St. Mary's Hospital, and Physician to the Hospital for Women.

These tumours are not mere hypertrophies but distinct growths. In simple hypertrophy there is, as you know, only an exaggeration of the normal tissue of the part, there is no foreign or adventitious element or structure; and I have been surprised to find in the writings of some authorities that all these uterine fibroids are regarded as made up only of the proper tissues of the uterus, variously combined. This, however, is not the case; in most if not in all of them, there are besides the ordinary elements of the uterus—namely, muscular fibre-cells in varying proportion, all bound together with firm connective tissue—a considerable proportion of cellular elements; in some places little else is observable, and the whole appearance there is very much the reverse of what one would be led to expect in a fibrous tumour. I have noted also that the proportion of these cellular elements varies directly with the rate of growth in the tumour—that is to say, that the more rapidly a tumour has grown, the less fibrous and the more cellular will it be found to be. This point is well seen in some of the specimens on the table, and their clinical histories bear out this view.

In number and size these tumours vary very greatly; sometimes only one is found, but this, according to my experience, is rather the exception than the rule. In one case I have seen the uterus completely studded with them, both within and without, so that on making a section of the uterine wall the

surface looked almost like the skin of a person suffering from small-pox; so numerous were the tumours that it was literally impossible to count them. They varied in size from that of a millet-seed, or even less, to that of a small walnut. I need not say how hopeless such a case would be from a therapeutical point of view, and how utterly impossible to diagnose it. A careful inspection of such a specimen as the one I show you here suggests the inquiry—How do these tumours originate? That in some way or other they are a consequence of mal-nutrition must, I think, be conceded. There seems no other way of accounting for them in the case to which I have referred just now, for where the disease is so general no other explanation meets the case. At each one of the innumerable points where the tumour exists in that specimen some departure from the ordinary cycle of nutritive changes must have been in operation. I think, too, we may go a step farther, and say that the change in question was one characterised rather by deficiency than excess of formative power; that, in fact, it is a degenerative process, and not a process of exaltation. The whole history of these growths supports this hypothesis, and the way in which they sometimes terminate, by decay and absorption, by calcareous degeneration, or by abscess, all these point to the same conclusion. Especially is this the case, I think, in the second of these processes, for the change in question consists "in an amorphous and disorderly deposit of the salts of lime and other bases, in combination with, or in the place of, the fibrous tissue." (Paget.)

It may be thought, at first sight, that the clinical history of uterine fibroids tends rather to contradict this statement, because there is no doubt that these tumours occur much more frequently at those periods of life when the uterine functions are in greatest activity. In this respect my experience coincides pretty closely with that of most other observers. For instance, of the 180 cases which have come under my care in the past seven years, 6 occurred between the ages of twenty and twenty-five inclusive, 14 from twenty-five to thirty, 42 from thirty to thirty-five, 33 from thirty-five to forty, 41 from forty to forty-five, 33 from forty-five to fifty, 9 from fifty to fifty-five, and 2 from fifty-five to sixty-five. Or to put them in decades, we find that 20 only occurred between the ages of twenty and thirty, 75 between thirty and forty, 74 between forty and fifty, and only 10 between fifty and sixty. Of course, the time at which these cases came under observation must not be taken as the date of occurrence of the growth; that must be put back at least for several years. But this only brings the fact of the origin of the tumour still more closely into relation with the period of greatest uterine activity. For instance,

allowing for a period of five years as that within which the tumour was growing from its very commencement as a mere speck, no larger than a pin's head, to the time when it first came under observation, the greatest number of cases (149 out of 180) must by this reckoning have originated between the ages of twenty-five and forty-five—a period which is universally admitted to include the highest functional activity. How, then, it may be asked, is this to be reconciled with the idea that these growths are the result of some form of mal-nutrition, and that the change in the formative power of the part affected is characterised rather by deficiency than excess?

A little reflection will, I think, satisfy us that the facts and the hypothesis are quite compatible; nay rather, that the latter *best* accords with the former. Let us look a little more closely into the facts. Of the total number of cases (180), 152 were married, 25 were single, and in 3 no note of the social condition in this respect is stated. Of the 152 who were married, 45 were sterile, 94 fertile. Of the latter, 46 aborted, the total abortions being 105. The number of children born was 454, and the total number of pregnancies was 559, giving an average of very nearly 6 pregnancies to each. Further, I find of the 152 who were married, the number of years of married life is stated in 128, and the total number of years amounts to 1937, giving an average of 15 years aud a fraction to each. All these facts are, I think, sufficient to prove that the period of greatest functional activity in the organ concerned is that within which the occurrence of fibroid tumours is most likely to happen. But functional activity—in other words, active nutritional change—implies corresponding waste, disintegration, decay; the two are commensurate one with the other, and it might, I suppose, be formulated as an axiom in pathology that, excluding blood poisons, such as those of gout, syphilis, the poisons of eruptive and other fevers, all of which have special affinities for particular tissues, parts which are subject to the greatest functional activity, and where the nutritive changes are consequently most actively carried on, are just those which are especially liable to disease. The greater the activity the greater is the waste, and the greater the waste the greater is the liability to disease. The uterus during procreative life is surely no exception to this rule; it is, in fact, the very basis of our practice, without which, I fear, our office of gynæcologists would be in danger of becoming a sinecure. There is no fear of that at present at any rate, as the experience of this hospital testifies. But, further, if you will look with your mental eye into the microscopical changes which we may suppose to be taking place in the minute spot where the germs, if I may so say, of a uterine fibroid tumour are being developed, you will

see, I think, indications of the kind of action I have pointed out. Suppose that, instead of this deficiency in the formative power of the local nutritional changes, there was an excess, surely what we should have would be, rapid growth, great pain, and a fumour characterised rather by softness than hardness. Such, at least, is the universal result in cases where excessive formative power is certainly going on, as is seen, for instance, in the case of certain forms of cancerous growths.

You must not suppose that all this is merely fanciful speculation with no practical interest or bearing. On the contrary, it is related in a very special manner to the treatment of these cases, at least in its medical aspect; and certainly no treatment can be successful which is not based upon sound pathological data. One illustration will suffice in proof of this statement, for I need only point to the difference in treatment between a case of uterine fibroid and one in cancer to demonstrate my meaning. In one word, local stimulation is the process upon which we shall mainly rely in the medical management of a case of fibroid tumour, where a cure is aimed at without the intervention of surgery, while all thought of such a process is most carefully excluded in the conduct of a case of cancer. Why? Because in the one case there is already an excess of formative activity which we are most anxious to curtail, while in the other an opposite condition exists, and local stimulation, with the slender hope of securing a removal of the disease by a process of absorption, is the key-note of our therapeutical system.

So much, then, on the pathology of these tumours. There are many other points in connexion with this branch of our subject upon which I should have been glad to remark had time permitted; some of these I shall perhaps allude to in considering the question of treatment. Before proceeding to discuss the *Symptoms*, a few words are necessary in regard to the classification of these growths which has been adopted by writers on the subject. The point is important, too, with reference especially to surgical measures, and hence the diagnosis of the several varieties is a matter of prime consideration. I do not propose, nor, indeed, is it necessary for me, to dwell at any length upon this part of our subject; suffice it to say, that for all practical purposes fibroid tumours of the uterus may be arranged in three classes, according to their situation and relation to the uterine walls. Thus we have, first, the *submucous* group, in which the tumour is originally developed immediately under the mucous surface. As these grow, they project more and more towards the uterine cavity, when they invade and distort to a greater or less extent, according to the size of the tumour. At first, and when extremely small, they

resemble very much another variety—viz., the interstitial, and they differ from them only in being nearer to the uterine cavity. Sometimes it is thought that, by the active efforts of the muscular fibres behind or beneath them, they are gradually projected from the bed in which they were originally developed; and as this process goes on, the contraction of the fibres behind gradually extrudes the growth, narrows its point of attachment, so that ultimately it becomes polypoidal in form, having a more or less slender stalk, and being covered all over by the uterine mucous membrane. In regard to this submucous group, the chief variations — and they are of great importance from a surgical point of view—have reference to their size, to the amount of their projection into the uterine cavity, and to the extent of their attachment.

Another variety of uterine fibroid is that in which the original development occurs, not near the mucous, but the peritoneal surface—the *subperitoneal* group. These are commonly believed—though upon what grounds I do not know, and it certainly does not agree with my experience—to be of more frequent occurrence than the former group; they give rise to very distinct symptoms, which usually mark them off very clearly from those we have just considered. In their growth it is thought that very much the same kind of action goes on with reference to the muscular fibres behind or beneath them as occurs in the case of the submucous group—that is to say, that a process of extrusion takes place by muscular effort, formed in both cases by the deficient resistance which exists upon either surface; so that here again the point of attachment may become gradually narrowed, and the tumour at last assume more or less of a pedunculate character. In a few rare cases, indeed, this process has been carried to such an extent that the tumour has at length become detached altogether, and, still maintaining its independent existence, has for a time lived free in the peritoneal cavity, and ultimately, in some cases, has taken root again, and grown in another part. Such a medical curiosity has, however, never fallen within my experience.

The third group of cases comprises those which belong to neither of the two classes just considered, but which lie as it were in the intervening territory, having originated in the substance of the uterine wall, not appreciably nearer to the one surface or the other. To these the term "interstitial," or intramural, fibroid has been given. This comprises a very large group of cases, and, according to my experience, they on the average attain to a greater size than either of the other two; though, of course, there are exceptions to this. The greater freedom of blood-supply is probably a sufficient explanation of the fact. In the growth of these tumours they may incline a

little more to one or other surface of the uterus, and in proportion as this occurs, so will the uterine cavity be more or less influenced, distorted, or enlarged—a point which obviously is important in a diagnostic point of view, and which is happily within our cognizance.

In reference to all these several varieties, it may be said that they occur much more frequently in the fundus and body of the uterus than in the cervix; indeed, it is very rarely that we meet with them in this latter situation, unless they have invaded that part in their growth downwards from the body. On the whole, I believe that the fundus is most frequently the seat of these growths, though I am unable to give you numerical data. In regard to their relative frequency in the anterior and posterior walls, I find that of 82 in which its position could be accurately defined, the tumour was situate in or upon the anterior wall 44 times, and in the posterior wall 38 times. In 22 other cases, it was situate five times on the right side, five times on the left, once only in the cervix; and in 11 cases it was distinctly superitoneal.

Treatment.—Before resorting to any plan of treatment, it is of course, above all things, necessary to determine, if possible, the exact position of the tumour; and, in reference to this point, you will bear in mind what I stated in my last lecture as being, on the whole, fairly deducible from a consideration of the leading symptoms—viz., that excessive hemorrhage, or other irregular discharge, is indicative of a polypoid, submucous, or interstitial fibroid, while great pain, on the other hand, results much more frequently from tumours which are subperitoneal, or which, if interstitial, are so placed as to cause obstruction to the menstrual flow or pressure on adjoining sensitive parts. As, however, the fitness of the case for medical or surgical treatment depends entirely, at least so far as our advice is concerned, upon a correct appreciation of the exact position of the tumour and its relation to the uterine walls, it is desirable that I should consider a little more fully the means at our command for determining this point. Of course if a case comes before us in which excessive menorrhagia, irregular attacks of hemorrhage, and profuse watery discharges are the leading symptoms, so that the patient has been reduced to a marked anæmic condition, and we find on examination that the uterus is occupied by a tumour, that the cervix is expanded over it equally all round, that its tissue is soft, the os a little patulous, its edges thin, and through it, though the opening be very small so as hardly to admit the tip of the finger, a tumour is felt projecting into the uterine cavity, no doubt whatever exists as to what ought to be done in such a case. Medicines, even of the palliative kind, are here quite out of

place, and the case is a perfect one for surgical interference by one or other of the methods I shall presently describe. Our advice should be given accordingly, and in no faltering tone.

Again, suppose a case presents itself similar to the foregoing as regards the general symptoms, with perhaps this difference, that, though menorrhagia is excessive, there is no history of irregular attacks of hemorrhage, and no profuse watery discharges, but that there is rather more pain in this than in the former case, and more disturbance of the functions of the neighbouring viscera—the bladder or rectum. The inference before vaginal examination will tend rather to the opinion that an interstitial fibroid exists. And if, on examining the uterus, we find its tissue hard, the cervix displaced from its central position in the pelvis, the os closed, or at least not patulous, and that a hard swelling exists in the pelvis, immediately connected with the uterus and opposite to the position of the cervix, most likely in the anterior or posterior wall of the uterus; if, further, on cautiously introducing the sound, we find that the uterine cavity is enlarged in all directions, that the sound seems to pass over a rounded surface, and that the slightest movement of the tumour at once affects the sound in utero, then there can be little doubt that we have to deal with a fibroid tumour of the interstitial kind, and the question of medical *versus* surgical treatment becomes one of pressing importance, and is at the same time full of great intricacy, difficulty, and danger. Such a case requires unquestionably the greatest skill, judgment, and discretion. It should be examined again and again before a definite course of action is decided on, at least if surgical interference is contemplated. And here I would urge very strongly the great importance of careful examination under chloroform before any surgical measures are attempted in any doubtful case. I do not think it is possible to gain a correct idea of the relations of an interstitial fibroid unless the patient is completely under the influence of chloroform, so that by combined internal and external examination, with and without the use of the sound, the whole position of the tumour can be mapped out, as in most cases it can with almost absolute certainty and accuracy. When this is done, then and and not until then shall we be in a position fairly to decide for our patient's welfare as to the course that should be adopted. I shall presently describe the surgical bearings of such a case, and the character of the operation suited to it.

Lastly, if a case occurs in which the leading symptom is pain in and about the pelvic region, with accompanying disturbance of the functions of the adjoining viscera—the bladder or rectum; with a certain but comparatively slight excess in the menstrual discharge; with no hemorrhage at irregular times,

and no amount of watery discharge; if, on examining such a case, we find much displacement of the cervix uteri, with but little alteration of its size, shape, or consistence, and that a tumour exists somewhere in the opposite direction to the cervix, causing its displacement; if, further, we find that the tumour is very mobile, and does not affect the cervix to the same extent as does an interstitial fibroid, showing that its connexion with the uterus is not so intimate in the one case as in the other; if, moreover, the uterine sound gives a measurement showing that the uterus is little if at all increased in size, —then the tumour, if uterine at all, is most probably of the subperitoneal form.

Having, then, determined accurately the position of the tumour, the question of its medical or surgical treatment is thereby settled; and, supposing that palliative treatment is required, our attention will probably be directed chiefly to the relief of one or other or all of the three leading symptoms—namely, menorrhagia, irregular discharge of mucus or of blood, and pain. Of these, undoubtedly, excessive discharge, either at or between the menstrual periods, will be the most common, and probably the most trying and troublesome symptom to treat. Of course I need not say that astringents will under such circumstances be called for; and, among the many that at different times enjoy popularity, it is difficult to select one which shall prove most effectual; indeed, I do not hesitate to declare that there is no single remedy that I know of which can be relied upon with anything like certainty to check the hemorrhage or arrest the menstrual flow in cases of this sort. I have tried, in turn, most, if not all, the so-called hæmostatics, and I cannot say of any one of them that it has been uniformly successful, or that it has not at times most signally failed. Gallic acid in one case, astringent chalybeates in another—and, of the latter, I know of none that is so effective as the ethereal peracetate of iron in half-drachm doses—acetate of lead in a third, taking care always, in giving the latter, to do so in solution with the dilute acetic acid; ergot of rye in a fourth, in half-drachm doses, giving this in the recent powder in preference to any other form;—these are, according to my experience, among the most efficient remedies of this class. But there is one that I have used during the last twelvemonth which certainly seems to me by far the most effective remedy of the kind that we possess: I mean the watery extract or liquor of the common periwinkle—the extractum vincæ major liquidum, as it is called. This, taken in drachm doses, properly diluted every four hours, I have seldom known to fail: it is certainly by far the most efficient remedy that I know of for the purpose in question. Why one remedy answers

at one time or in one case better than another I am quite unable to say; constitutional or local peculiarity may have to do with it; but in any case I can lay down no rule for your guidance, for the whole thing seems to be purely empirical. Of those agents which, while possessing no astringency, have nevertheless in some hands proved effective as hæmostatics, such as digitalis, Indian hemp, turpentine, and mercury, I have no satisfactory opinion to offer, for the reason that they have all in my hands proved utter failures. Of course, where great anæmia exists, a chalybeate astringent would seem the most fitting, but I have on many occasions found it rather increase than diminish the bleeding. The same may be said also of ergot. Dr. M'Clintock speaks very highly of the hæmostatic properties of mercury, confirming in this respect the experience of Dr. Tanner, who found it a most useful agent for this purpose. I must own that I have never seen such a result. If I were to place the medicines in the order in which I have found them of greatest value, I should assign them as follows: the liquid extract of the common garden periwinkle, the ethereal peracetate of iron, ergot in the form presently to be described, gallic and sulphuric acids with the compound infusion of roses, the acetate of lead with dilute acetic acid, and, in a few rare cases of the congestive kind, the local abstraction of blood by means of leeches to the cervix; these have all, in turns, proved efficient in my hands. As a general rule, I have observed that when hemorrhage does occur, which is very seldom, in cases of subperitoneal fibroids, it is more easily arrested than in either of the other varieties, and for such the periwinkle, or the chloride of calcium, given perseveringly for months, has been the most successful. In interstitial fibroids the bleeding is more difficult to control, and ergot of rye or borax has proved the most efficient agent in such cases. In the submucous, and still more in the polypoidal forms, the difficulty of arresting hemorrhage is greater than in either of the others; here the vinca major and the more common astringents, such as gallic acid, sulphuric acid, iron alum, or the peracetate of iron, have appeared to me to answer best.

There is one other remedy which I have tried quite recently in three cases, and the result in one of these was very marked; I allude to the subcutaneous injection of ergotine. The solution which I employed contained ten grains of ergotine to a drachm, and from five to twenty minims were injected each day. In one case severe cellular inflammation followed in the site of the puncture, so that the treatment had to be discontinued; in another it excited so much pain and distress about the præcordia that I was obliged to desist, and in neither did any good result; while in a third the remedy was continued

daily for ten days, and not only did it produce a decidedly beneficial result in regard to the next menstrual period, but the hemorrhage was restrained for two months afterwards, when the case passed from my observation. I believe that we have in this agent a powerful hæmostatic remedy, for I am informed that very good results have followed its practice in other hands.

In addition to the above, the local application of astringents will often prove most successful, and these we may apply either in the shape of vaginal suppositories of tannin, or the glycerine of tannin, or matico, or alum, or we may inject the uterus with perchloride of iron, though I very much prefer the use of the anhydrous zinc points as suggested by Dr. Braxton Hicks. In the more severe cases, where a fatal issue seems almost to threaten, it will be necessary to resort to the use of the tampon.

Besides all these remedies there is a measure which has been greatly extolled by some as most efficient in arresting hemorrhage—viz., the division of the cervix. Various explanations have been given to account for this result, but none of them, I think, are satisfactory, nor has the proceeding itself proved successful in my hands. Occasionally, however, where the cervix has been dilated with sponge tents, and some inflammation has followed in consequence, the disposition to hemorrhage has been afterwards completely arrested. It seems likely, therefore, that the good result which occasionally flows from dividing the cervix is to be explained in the same way. A similar method to this has been recommended by Dr. Atlee, of Philadelphia, which he describes as most successful. His plan is "to pass a bistoury along the vagina into the cavity of the uterus, and make a very free incision into the most exposed portion of the tumour." The operation is to be done during the occurrence of hemorrhage, and he declares that it is "invariably arrested instantaneously." I have had no experience of this method, but anything coming with the approval of Dr. Atlee is deserving of every attention. So much, then, for the question of hemorrhage, or excessive discharge of any kind, in cases of uterine fibroid.

In regard to the relief of pain, there are several ways open to us by which we may hope to accomplish this end; and we shall be probably guided to the right method according to the correctness of our appreciation of its causes. Sometimes the pain is produced mechanically, by pressure of the tumour on neighbouring parts. This is especially likely to occur in the subperitoneal and pedunculate variety; and in such cases great relief has occasionally followed the removal of the tumour by the hand from one part to another. In one case that came under my care where the tumour was situate posteriorly, the

introduction of an air-pessary behind the cervix proved very successful in supporting the tumour, and gave great relief to the patient. In a movable tumour, therefore, the possibility of a mechanical cause of pain should be borne in mind. The pain which is due to a congested or inflamed condition of the tumour must be met by the remedies already described when treating of this condition. And in cases where neither of these two causes is in operation, but where the pain is, nevertheless, constant and wearing, though its cause may not be very clearly defined, there we must resort to the employment of anodynes; and these we may use either subcutaneously or by the mouth, or better still, I think, by the vagina. I will only speak of the latter mode of treatment, because, of course, the two former are well understood. The advantages of this method are—first, that the anodyne is applied directly to the affected nerves; and, secondly, that the stomach and digestive organs are not so injuriously affected as when the same drug is given by the mouth. On the other hand, the disadvantages are—first, what I may call the moral or sentimental objection to the practice of daily introducing a foreign substance into the vagina; and, secondly, the discharge which must necessarily take place of those portions of the pessary which are not absorbed. This latter objection formerly applied with much greater force when the greasy substance—cocoa-butter—was used, than it does now, because, as I showed long ago, in a paper which appeared in the Practitioner, "On the Use of Medicated Vaginal Pessaries," it is no part of the function of the vagina to digest fats; and the presence of such a greasy substance in the vagina tends to prevent the absorption of the active ingredients. Moreover, the discharge of all the unabsorbable part, including the whole basis of the pessary, is, in private practice, very objectionable. I have, therefore, long since discarded the use of the cocoa-butter, at least in all cases where an astringent is not required; and in its place I now use, as the basis of the pessary, a mixture of gelatine and glycerine, in the proportion of one part of the former to four of the latter. This makes an admirable mass; it readily melts at the temperature of the vagina, and any ingredient that is not astringent can be added to it; but as all astringents combine with the gelatine to form an insoluble compound, it cannot be used in these cases. There is one other point I may mention in connection with these pessaries—namely, their size. Very commonly they are used as large as a Minie ball, or even larger. There is no necessity for this, and I think it is on every account desirable that they should be as small as possible. I therefore always use them the size of a suppository, which is quite large enough to carry any drug you may require

Another advantage of using the gelatine and glycerine is, that the latter promotes the secretion of the vaginal mucous membrane, and so favours the absorption of the active ingredient. The anodynes which I have found most useful are the alkaloids coneia, atropia, and morphia—one to two grains of coneia, one-eighteenth to one-twelfth of a grain of atropia, and half a grain to a grain of morphia. These may be used once or twice a day, according to circumstances. On the whole I am inclined to believe that coneia is by far the best and most efficient anodyne we possess for vaginal purposes. I am satisfied that it is in all ovarian cases, but am not quite so sure of it in the class of cases we are considering.

So much, then, for what we must call only the palliative treatment of these tumours. Happily we are not restricted to this, for we possess many means by which we can effectually remove these growths. The mode of procedure to be adopted in any given case must of course depend upon the nature of the case, and especially upon the situation of the tumour. Hence the great importance of being able to diagnose accurately the position of the tumour, and its relation to the uterine walls. In the polypoid and in the submucous varieties our course of action is clearly marked out; in both cases the first step is to secure free dilatation of the cervix, so as at least to admit easily two fingers within the uterine cavity, in a case where the tumour is small, and has a distinct pedicle; while in others where the tumour is large, and has an extensve attachment, though only to the mucous surface, there we must dilate the cervix more freely by means first of the sponge or sea-tangle tents, and afterwards with the india-rubber air- or water-bags, so as to admit not merely two or three fingers, but even in some cases the entire hand. I have succeeded in doing this on several occasions, detaching thereby tumours considerably larger than a fœtal head at full term, and separating them completely from the uterus by means of the écraseur. In one case, after cutting off such a tumour I had the greatest difficulty in extracting it from the uterus, and was obliged to use a pair of long obstetric forceps for this purpose. We shall generally find, at least I have hitherto found, little or no difficulty in separating a submucous tumour from its attachment; as a rule, one or more of the fingers is quite sufficient for this purpose, except at one part where it will be found to have a firmer attachment than elsewhere, and there the écraseur must be used. The old-fashioned canula of Gooch is now, I think, pretty generally abandoned, and deservedly so, for there is nothing to be said in its favour, and very much may be fairly urged against it. It is, I think, most objectionable practice to retain, as with the use of this instrument we are obliged often

to do, a sloughing putrid mass in the vagina, or even in the uterus, sometimes for days together, until the ligature, which is here used for the purpose of strangulation, has succeeded in cutting its way through the tumour. I am in the habit always of using, and greatly prefer, the single wire écraseur, The wire in question is the same as is used in pianofortes; it is extremely tough, but is most conveniently stiff and at the same time springy, so that the loop may be nipped tightly and narrowed for facility of passage through the vagina and os uteri, but as soon as it gets beyond this into the cavity of the uterus it springs open so as to encircle even a large tumour, and its stiffness makes it very easy of application because it can be passed, of course with all due care and gentleness, along the uterine wall, even without the guide of the finger, if the operator is accustomed to such manipulations, and so up to the stalk or base of the tumour. While this is being done, and also when the screw is being worked so as to fix the wire round the tumour, the instrument should be constantly semi-rotated, so as to free the wire from the uterus, at the same time that the point is pressed home as far as it will go. Occasionally when there is much difficulty in passing the wire up to the fundus, I have used a small hook as a carrier of the wire to the point sought. The application of the wire is, I think, easier if we use a new wire for every operation; it is inexpensive, and it is much easier to use than when the wire has before been drawn through the instrument, and so spoilt its round shape and springiness. I usually cut through the tumour quickly. There is seldom or never any bleeding of consequence; if there be, I plug with cotton wool and tincture of matico and a little carbolic acid.

With reference to the subperitoneal fibroids, I can conceive no justification for surgical interference, except it be in cases where the suffering caused by the tumour is so great, or the condition so alarming, that life is either insupportable from pain and general distress or is threatened by the growth of the tumour. Even in these cases there ought to be at least a fair presumption that the tumour is removable by the operation of gastrotomy. But while I would deprecate any hasty resort to operative interference in these cases, I must at the same time express again my opinion, as I have done on more than one occasion in writing, that "many of these cases might be attacked which are now left alone to the great distress of the patient; and I cannot help thinking that, with our rapidly diminishing mortality after ovariotomy, due in great measure to the knowledge we are gaining from daily accumulating experience, the time will come, and is fast approaching, when we shall be justified in attempting the removal of many of these growths by abdominal section." I regret that I have

not more time to discuss this question, but the subject has been fairly and ably handled by Professor Thomas, of New York, in the last edition of his admirable "Treatise on the Diseases of Women."

Between the two varieties of tumour the treatment of which we have considered—viz., the submucous and the subperitoneal—there is a large group, indeed, as I believe, by far the largest group, of cases—namely, the interstitial; and it is to these that the attention of the profession will, I am sure, be more and more directed in future. Since I began to attack these growths by the process of enucleation I have been surprised often at the facility with which they may sometimes be removed, and the comparatively slight constitutional disturbance which their removal has involved. At the same time I fully recognise the danger that may be run, and the serious and even fatal consequences that may ensue, unless, on the one hand we are very precise and clear in our diagnosis of the exact position of the tumour, and in its relation to the uterine walls, especially on its peritoneal aspect; and, on the other hand, are extremely careful, and, as far as possible, gentle in the somewhat rough handling to which the uterus must necessarily be subjected in the process of enucleation. Hitherto the results in my hands have proved so successful that I do not hesitate to recommend its more frequent adoption, even though so great an authority as Dr. M'Clintock expresses an adverse opinion upon it. It was, indeed, to a great extent due to the strong objections urged by him and other writers that I was for a long time deterred from resorting to this operation. Dr. M'Clintock's opinion is thus expressed: "The results of this operation are very far from encouraging, and it may be doubted whether it will ever become an established mode of practice. It is attended by grave dangers of an immediate and remote kind—viz., hemorrhage and pyæmia. The idea of the operation suggested itself to Velpeau from observing the looseness of attachment between some of these tumours and the uterus. But in my experience it is quite exceptional to find the connexions of the tumour with the uterus so lax as to admit of its ready enucleation or dislodgment; while in most cases very great and direct force would have been required to effect this; and in some instances, as Dr. Rigby remarks, the tumour seems to be almost *continuous* with the uterine substance, so that its detachment would be next to an impossibility. However," he adds, "this operation should not be wholly proscribed." Dr. West, again, expresses himself still more strongly to the same effect. He states "that twenty-seven operations yield fourteen deaths to thirteen recoveries, while in four of the latter the operation was incomplete, and a portion of the tumour was

left behind." Later on he expresses the opinion that, if we could get correct statistics of the operation, it would be shown that most of the cases have ended fatally; and, accordingly, he classes the operation "among the most hazardous in surgery," and concludes by saying that the "hazard attendant on operations for the enucleation of interstitial fibrous tumours of the uterus, when still imbedded in the walls of the organ, is so extreme as to render them generally inadmissible, and to remove them to that class of exceptional proceedings which the special condition of the patient and the unusual dexterity of the surgeon alone justify." Now, I cannot agree with either of these two authorities, for I am sure from my own experience that they have greatly exaggerated the dangers that are incurred, if only due care is taken in the selection of cases suitable for the operation. You can readily understand that an interstitial fibroid, not perhaps of very great size, may yet be so placed that only the very thinnest layer of uterine tissue covers it on its peritoneal side; and, of course, in such a case very great danger will be run in attempting its enucleation. On the other hand, you may have a fibroid equally interstitial, of large or small size, completely covered with uterine tissue, but with a good thick layer upon its peritoneal surface, and a comparatively thin layer on its mucous surface. Obviously the danger in these two cases is widely different, and in the latter it is comparatively slight. How, then, is the differential diagnosis of these two conditions to be made, at least with some approach to accuracy? For, of course, accuracy in such a matter is of the very utmost importance. I have already considered this subject in part when speaking of the differential diagnosis of the several varities of uterine fibroids. It must be judged of partly by symptoms and partly by physical signs, and, as a general rule, it will be found that the more subperitoneal a tumour is, the less will the size and form of the uterine cavity be interfered with; while on the other hand, the more submucous it is, the more will those relations be distorted; so that where the hemorrhage or other uterine discharge is very free, and where the size of the uterine cavity, as measured by the uterine sound, bears a fair proportion to the size of the tumour, there we may reasonably expect to find a fair amount of uterine tissue on the peritoneal surface of the tumour, a thinner layer on its mucous surface, and, consequently, the case is a favourable one for enucleation. I should add, too, that the lower the tumour is in the pelvis, the more the body of the uterus is involved; and the nearer the tumour is to the cervix, still more if the cervix itself is invaded by it, the greater are the chances of successful interference. A long cervix, especially if it be little or not at all altered from its

natural size and shape, must be regarded as unfavourable for surgical treatment in cases uterine fibroids.

I regret that I have not more time for the consideration of this part of our subject; but I have left myself only a few minutes for a description of the operation, and cannot, I fear, detail the cases I hoped to bring before you where I have resorted to this proceeding. Now the operation of enucleation should be conducted upon a definite plan, and according to some fixed principle, and both may, I think, be summed up as follows :—First, that these uterine tumours should be regarded as essentially foreign bodies. Secondly, that nature's method of dealing with these foreign bodies is to expel them; we see this often in the case of polypi, and occasionally even in large submucous fibroids. Thirdly, that for her success in this matter a dilated or dilatable os, and uterine contraction, are the only essential requisites. Fourthly, that it is the duty of the physician, when these conditions are absent, to step in with his art and imitate the process. Fifthly, that for this purpose the cervix must be opened up, and the uterus be stimulated to contraction. Lastly, that both these objects will be greatly facilitated, in the case of interstitial tumours, by their forcible detachment from the position in which they are embedded, thus making them more and more foreign to the organ where they are lodged.

Now as to details. The conditions of success are, as I have said, an open os, uterine contraction, and detachment of the tumour. As to the first, I am strongly opposed to the method of dilatation, and in favour of free division. I have seen infinitely greater evils result from dilatation than from division. Moreover, the former is inadequate to the subsequent steps of the operation, because—and this applies especially to cases where the tumour is large—I think it is most important that the process of detachment should be conducted gradually, and in successive portions, so as not at one time to expose a very large surface to the risk of inflammation and purulent absorption. If the cervix be divided, it is done once for all, and is ready for the successive steps of the operation; whereas in dilatation the same process has to be repeated every time that we resort to detachment.

My plan, then, is, first to divide the cervix freely in two or three places, taking care to prevent reunion of the divided surface, and always to plug the vagina for a few hours after the division is made. In about a week, or at most a fortnight, from this time, supposing that all has gone on well, I introduce the finger up to the tumour, and, with whatever force is necessary (sometimes a good deal is required, sometimes but very little), I break through the tissue covering the tumour at

the point where it joins the healthy uterus. Once within what I call the intracapsular space—that is, the space between the tumour and the uterus,—there is, according to my experience, very little difficulty indeed in passing the finger freely round the tumour in all directions, breaking down its loose cellular attachments, and, in short, shelling it out from its uterine bed. As I have said, I greatly prefer not doing all this at once, but in successive steps. When the tumour is in good part detached, but not before then, we may advance to the next step in the process, and endeavour to procure its expulsion. This we do by the administration of any of the various oxytoxic agents, of which ergot and borax are certainly the best. Sometimes I have used galvanism, with evident success; but in this matter I do not think it is of much importance what we use, for, if the tumour is only well separated, the uterus will usually contract most efficiently, and all the more readily, no doubt, because of the irritation to which it has been subjected by the previous operations. The last step in the operation is the removal of the tumour by means of the écraseur, in the same way as has been already recommended in the case of polypoid growths. I have once succeeded in entirely enucleating such a tumour without the use of the écraseur; and I do not at all see why in some cases we should not be able completely to detach it with the finger, just as I have done again and again in post-mortem cases. Where we are dealing with a very large tumour, it may occasionally be necessary to remove it in successive portions. I had a case of this kind under my care some time ago: the uterus was occupied by one of these interstitial fibroids, and was of such a size that the fundus reached fully up to the umbilicus. In that case, after I had detached a good part of the tumour, the uterus contracted so vigorously upon it that in four days a large mass was forced into the vagina, and I was compelled to remove it before waiting for the subsequent separation and removal of the remaining portion. Ultimately, however, I got it all away, and the patient came to me the other day perfectly well.

Such, then, is the plan of enucleation which I am in the habit of practising, which I recommend strongly as by far the best way of treating these cases of interstitial fibroid, and which, if conducted with all due care and in properly selected cases, is, I am convinced, not nearly so dangerous an operation as it has been represented.

If you will pardon my detaining you a little longer, there is just one other method of treating these cases about which I should like to say a few words. It is a method which was discovered, I believe quite accidentally, by my friend Dr. Greenhalgh; and it consists in the application of the actual

cautery directly to the tumour itself. Dr. Greenhalgh was engaged one day in some cutting operation upon a rather large interstitial fibroid, when the hemorrhage became so severe that he was obliged to desist; and as all styptics failed to arrest the bleeding, he applied the actual cautery so freely to the bleeding surface that he burnt quite a hole in the tumour. The bleeding stopped, and, to his great surprise, he found in the course of a few days that the tumour was protruding forcibly through the opening made by the cautery. Little by little the tumour descended, until the whole mass was eventually extruded, having been but slightly assisted by art. Encouraged by this, he in a subsequent case determined to repeat the experiment. I was requested to meet him in consultation, and was present when the operation was performed. The case was one of a large interstitial fibroid, growing entirely in the posterior wall of the uterus, and invading the cervix so completely that its posterior lip formed part of the tumour, and was, in fact, expanded over it; the tumour itself occupied three-fourths of the pelvis posteriorly, the os and anterior wall of the uterus being thrust forward against the symphysis pubis. A small conically bulbous cautery was thrust through the vaginal roof and into the centre of the tumour, about an inch or so behind the os uteri, to a depth of about an inch or an inch and a half. No bleeding followed the operation. In the course of a few days the opening thus made began to dilate, and the finger, when passed through it, readily detected a space between the tumour and the posterior wall of the uterus, where detachment could be easily effected. Dr. Greenhalgh being obliged to leave London for the Continent, I was requested to take charge of the case. At that time, which was about a week after the application of the cautery, the opening was fully as large as a crown-piece, and through it the tumour was beginning to protrude. All this, too, by the natural effort, and without the help of any drug. In four or five days more, during which time I had aided the process of expulsion by daily detaching it more and more, the mass came to occupy the whole of the vaginal cavity, and began to assume a sloughy condition, from which serious constitutional disturbance threatened. I therefore applied the écraseur, and removed all that was then practicable. A week after this Dr. Greenhalgh returned, and by that time more of the tumour had been extruded. The patient was accordingly placed under the influence of chloroform; and then, without the slightest difficulty, the entire mass was turned out with the finger, no other instrument being used. The patient made a completely satisfactory recovery.

Such a case is highly instructive from several points of view;

it seems to open up an entirely new plan of treating these growths, and one which, I think, has very much to commend it. It avoids the danger of hemorrhage in the cutting operations, and may possibly prove preventive of purulent infection; while it seems also in some way to promote uterine contraction and expulsion of the tumour. It is, however, of course very limited in its application; for it can only be resorted to in cases where the tumour is close down upon the vaginal roof and has entirely invaded the cervix.—*Lancet, May* 10 *and July* 12, 1873, *pp.* 657, 35.

105.—CASES ILLUSTRATIVE OF PERSISTENCE OF THE HYMEN.

By Dr. JAMES ST. CLAIR GRAY, Assistant to the Professor of Physiology, Glasgow University.

The following cases, while presenting to the obstetrician much matter of interest, are of still greater importance to the medico-legalist, as they afford incontestible proof that persistence of the hymen is compatible with the wedded state, that its destruction does not necessarily follow even the calling of a prostitute, and hence, that its persistence after attempted rape must not be relied upon solely as evidence sufficient to disprove the charge.

Case 1.—A lady, aged 43 years, who had spent twenty-four years in the matrimonial condition, suffered from a swelling in the right iliac fossa, which, on careful examination by external palpation, appeared to be connected with the ovary of that side. In order, then, to ascertain exactly its relation to this and the other pelvic viscera, a vaginal examination was attempted, but at first no vaginal orifice could be discovered, the meatus being guarded by a perfect hymen, having anteriorly a small aperture which permitted the escape of the menstrual flow. Careful examination further revealed a slight depression involving the anterior portion of the perinæum and the posterior portion of the hymen. This depression, evidently the result of frequent attempts at coitus, was, under pressure by the finger, capable only of affording a cul-de-sac not more than three-quarters of an inch in depth, while its normal depth was but one-quarter of an inch.

Case 2.—In this case the condition of parts observed in Case 1 was also observed, but was greater in degree. A lady, aged 30 years, who had been married ten years, but was childless. She complained of all the symptoms indicative of prolapsus uteri. On a vaginal examination being made, the os uteri was found situated about an inch from the meatus. The os uteri presented to the finger a very ill-defined outline; and this, o

careful examination, was found to be due to the intervention of a thin membrane between that organ and the finger. This membrane was then discovered to be but a persistent hymen, which, from frequent attempts at sexual intercourse, had become pouched, so as to give rise to the formation of a perfect cul-de-sac, which, in its normal state, was from three-quarters of an inch to one inch in depth, but under pressure, either by the finger or the speculum, was capable of being distended to the extent of about one inch and a half. This cul-de-sac, as in the former case, involved the posterior two-thirds or rather more of the hymen and the anterior portion of the perinæum, and its persistence was evidently due to the presence in the tissue of strong bands of fibrous or fibro-elastic tissue, which rendered the structure as a whole very resilient.

In Case 1 no steps were taken to destroy the hymen, owing to the comparatively advanced age of the patient, as also to the fact that should pregnancy have followed (which, while improbable, was yet possible), the ovarian tumour might have led to a complication of no little gravity. Bromide of potassium was given in large doses, with a view to decrease the size of the ovarian tumour, or, at least, to prevent its further development. This treatment was continued for several months, the tumour being at the expiry of that time rather smaller than formerly. When last seen, patient was still much in the same condition. Her husband was a robust man, weighing 14 stone, and was in every respect well formed.

In Case 2 the patient was well placed under the influence of chloroform, and the hymen was then forcibly ruptured by means of a speculum. The uterus was then found to be not only prolapsed, but also retroverted and slightly retroflexed. The uterus was replaced in its normal situation, and there secured by means of a Hodge's pessary. In a few days, all inconvenience arising from the operation had subsided, and the symptoms due to the prolapsus had entirely disappeared.

These two cases are then, I think, sufficient to prove that, even under circumstances the most favourable for its rupture, the hymen may remain intact, and if we remember that in cases of attempted rape the circumstances are less favourable—any struggling on the part of the female having a tendency to prevent perfect penetration—we are, I think, warranted in inferring that in such cases the proportion of those in which the hymen is not destroyed must be considerable.

Still further, however, to strengthen this position, I shall now shortly relate the particulars of three cases (3, 4, and 5) in which the hymen was found persistent in prostitutes. These three cases were found among about 1500, who within

the last few years, applied for admission to the Glasgow Magdalene Asylum, Lochburn, Maryhill, and should subsequent experience establish this as the precise proportion, viz., 1 in 500, it will considerably affect the significance of persistence of the hymen as an evidence or a sign of virginity.

Case 3.—A. B., aged about 25 years, and for seven years a prostitute, applied for admission to the Magdalene Asylum. With a view to ascertain whether or not she suffered from any venereal or other disease of an infectious or contagious nature, she was subjected to the usual examination. In every respect she was healthy; but the hymen was perfectly entire, save anteriorly where there was a small aperture, barely sufficient to permit the passage of a crow-quill. She stated that she had never suffered any inconvenience during her menstrual periods, that she had never contracted any venereal disease, and that she was perfectly unaware of there being anything abnormal or unusual in her conformation. In this case the hymen was firm but resilient, containing evidently numerous elastic and fibro-elastic bands.

Case 4.—C. B., aged about 24 years, also a prostitute, had adopted this path of life at the age of 16 years. Having likewise presented herself for admission to the G. M. A., was, on examination, found perfectly free from all disease; but presented a persistent hymen of a soft, but strongly resilient consistence, indeed, so much so was this the case that on making pressure with the finger, it afforded the sensation of small reduplications of mucous membrane being gradually thrust aside, but careful examination established the case as one of persistent hymen, in which the usually thin crescentic duplicature of membrane was greatly modified by the deposit in its substance of numerous muscular bands. She, like her predecessor, had never contracted any venereal disease, suffered no inconvenience during menstruation, and was perfectly unaware of there being any unusual condition of parts.

Case 5.—Like the preceding cases, this person, D. M., aged 30, had been a prostitute for 11 years, and applied for admission to the G. M. A., wishing to alter her mode of life. She had never suffered from any venereal affection, and presented a perfectly well-defined crescentic hymen of the usual size, but of very dense consistence—indeed, to the touch it seemed almost cartilaginous.

These cases are full of significance pointing most distinctly to the fact that the persistence of the hymen, *per se* as a sign of virginity, must be viewed with great care, while it must, at the same time, be taken as one of the class of signs rather collateral than direct in influencing the decision as to whether connection has, or has not taken place, or at least been attempted.

As adding still further proof in favour of the occasional occurrence of this rather abnormal condition, I have but to refer to one case which has come under observation, and which adds another to those already on record in which conception has taken place without rupture of the hymen.

These cases may be divided into those occurring in unmarried and those occurring in married females. In the majority of cases included in the first class, it appears that an explanation may be found in the fact that popular opinion holds it impossible for a conception to occur without full penetration; and hence a certain degree only of penetration is in these cases admitted, and thus, if the hymen be at all resilient, it may resist the slight pressure made upon it, while by means of the small anterior aperture the fructifying element may find its way to the cavity of the uterus, and thence to the ovum. But in the case of married females, constituting the second and smaller section, no such explanation will hold, and we are hence shut up to cases of what might, for want of a better appellation, be termed *genuine persistent hymen*, due to abnormal development of tissue in its structure. It is to this latter class that the following case belongs:—

Case 6.—Mrs. A., aged 29 years, had been married seven years before this her first confinement. When first seen she had been in labour about twelve hours. Vaginal examination being attempted failed to reveal any external orifice, and this rather peculiar phenomenon at once received explanation when a persistent hymen was discovered of such size and form as completely to occlude the meatus, save in its central part, where a small aperture was discovered sufficient in size to admit an ordinary sized crow-quill. She had no idea that such was the condition of parts, and seemed to have suffered no inconvenience during menstruation. Labour was tedious, but in the course of forty-eight hours the head of the child was found to be fully distending the perinæum. A crucial incision was then made through the hymen, and delivery effected by the use of the forceps, this being necessitated by the exhausted state in which the patient now was. Recovery was rapid, and the child, though born asphyxiated, was resuscitated. Mother and child are at present alive and well.

Such are examples of persistent hymen in which the persistence is due to an adventitious development of elastic, fibro-elastic, or muscular tissue. But there is another cause which may lead to the presence in the married female of a perfect hymen. The cause to which I refer has, by Dr. Marion Sims, been termed *vaginismus*, and this affection may be defined as a peculiar state of parts whereby, either from excessive nervous supply, or a peculiar irritable condition of the normal supply,

any attempt to consummate the sexual act, or indeed, any pressure made in the vicinity, causes the infliction of intolerable pain upon the female. This is an affection certainly much more frequently met with in females in whom the hymen has been destroyed, and in whom the seat of the affection is rather the carunculæ myrtiformes, but the cases which I have here to record are examples of the affection conjoined with perfect development of the hymen.

Case 7.—Mrs. B., aged 38 years, had been married thirteen years. Her husband stated that whenever he attempted performance of the sexual act, she suffered excessive pain and implored him to desist. She was in every other respect healthy. When first seen, patient seemed anxious to be cured, and had no hesitation in submitting to a vaginal examination, but no sooner did the finger touch the vulva than she cried out loudly, and sprang to the opposite side of the bed. After various unsuccessful attempts, it was arranged to visit again in about eight days, when chloroform should be administered, and the necessary proceedings adopted. On calling the following week, she objected to be placed under the influence of chloroform, and said she would much rather endure the pain, as she was firmly convinced she would not recover from the effects of the anæsthetic. An attempt was accordingly made to proceed without it, but beyond ascertaining that the hymen was perfect, nothing further could be learned, and no operation performed, on account of the intense suffering which the least manipulation of the parts induced. Since then she has been frequently seen, but she is still in the same condition as previous to her *formal* marriage, an event of fully eighteen years ago. Query. According to Scotch Law, was marriage in this case consummated, and might a degree of nullity of marriage be obtained?

The two following cases are perhaps more interesting, as in both the obstacle was removed, and the operative interference was rapidly followed by conception.

Case 8.—Mrs. N., aged about 25 years, had been married four years. She stated that since her marriage sexual intercourse had been entirely precluded by the extreme degree of suffering induced by any attempt at its performance. General health perfect. On examination the parts were found very tender and sensitive, the very slightest pressure occasioning intense suffering. Patient being placed under the influence of chloroform, there was found in the centre of a persistent hymen, which was tense and unyielding, an aperture about the size of a crow quill. A bougie was thrust through the hymen, then one of larger size, then a small thick-glass speculum, and ultimately one of medium diameter. After the lapse of eight days, chloroform was again administered, and the dilatation continued till the largest size

speculum was introduced. Patient complained a little of pain, but this was perfectly relieved by the use of the atropine pessary. Within a fortnight recovery was perfect; she returned to her husband in Marseilles, and has since given birth to four children.

Case 9.—Mrs. M'N. has been married seven years. In all respects has enjoyed good health, but complains that any attempt at sexual connection is accompanied by pain so excessive as at once to necessitate desistance therefrom. On examination the hymen was found entire, with a small fissure about half-an-inch long in a slightly oblique direction. Here, also, as in most of the preceding cases, there was a small aperture anteriorly, by means of which the menstrual discharge obtained egress. Chloroform was administered, and an incision made through the back or fissure, after which specula of various diameters were introduced, thereby perfectly destroying the hymen. In a few days recovery was perfect, and she is now the mother of three children.

These, then, may be taken as typical cases of vaginismus, an affection which may be said to be of recent introduction into the literature of obstetrics, but one which deserves the close attention of the obstetrician. To these I might have added examples of cases of the same affection, but affecting persons in whom rupture of the hymen had already occurred, but, as such cases do not illustrate the subject in hand, I reserve them for future matter of remark.—*Glasgow Medical Journal, May*, 1873, *p.* 346.

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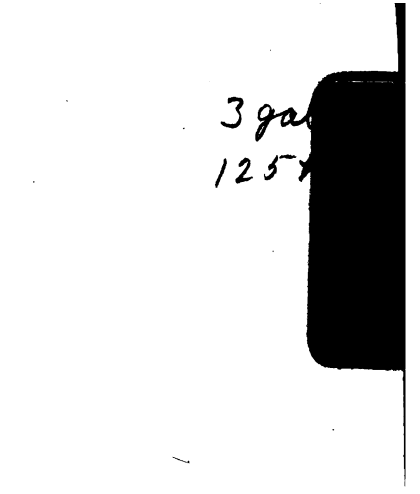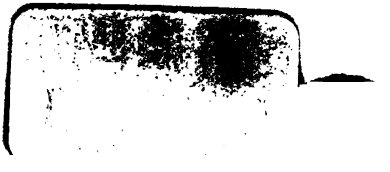
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